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Volume 9 Number 4 October 1959

The Psychological Record

a quarterly journal in theoretical and experimental psychology

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Published at DENISON UNIVERSITY, Granville, Ohio

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THE PSYCHOLOGICAL RECORD is a non-profit publication. It is published quarterly, in January, April, July, and October, at Denison University, Granville, Ohio. Subscription price is $4.00 a year (APA members—$3.00; students—$1.50).

With the permission of the Principia Press, Inc., THE PSYCHOLOGICAL RECORD is a continuation of the journal formerly published under this title. Publication of THE PSYCHOLOGICAL RECORD was resumed in January, 1956.

As presently organized THE PSYCHOLOGICAL RECORD publishes both theoretical and experimental articles, commentary on current developments in psychology, and descriptions of research planned or in progress. The journal is designed to serve *a critical function in psychology*. It therefore favors the publication of papers that develop new approaches to the study of behavior and new methodologies, and which undertake critiques of existing approaches and methods.

Articles should be prepared according to the form suggested for APA publications (*APA Publication Manual*) and submitted in duplicate to the Editor. The author cost per page is $3.00. There is an additional author charge for cuts and special composition. Reprints are available at cost.

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The Psychological Record, 1959, 9, 121-130.

PERSPECTIVES IN PSYCHOLOGY

XII. BEHAVIOR AS AN INDEPENDENT VARIABLE

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Three kinds of variables are recognized in the relationships of a scientific system: independent, dependent, and intervening. The dependent variables are used for the description of the subject matter under observation; the independent variables refer to the controlled and manipulated conditions of the experiment; intervening variables are reserved for the construction of theories, and are not necessary for empirical relationships. The so-called controlled and uncontrolled variables concern the experimental conditions, and both may be considered as independent. What remains for an empirical relationship is the paradigm expressed in terms only of dependent and independent variables

$$DV = f(IV_1, IV_2, \ldots IV_N)$$

Many psychologists, particularly those of the S-R schools, will say that when psychology is at its best, the DV's are identified with *behavior*, while the IV's are identified with *environmental* or *stimulating conditions*. The above equation is verbalized as "Behavior is a function of the stimulating conditions", or in symbols, $R = f(S)$. Kimble (1953) argues for this paradigm with lucidity, and it is clear that Spence (1956) and Estes (1950) also prefer to restrict the variables in "good" psychological laws in such a fashion. This is the so-called *S-R formula.*

Another type of law is recognized by this school, even tolerated, though not condoned: this is the *R-R formula,* where both the IV and DV refer to behaviors, or $R_1 = f(R_2)$. One gathers that the R-R law is considered at best a stepping stone, at worst a dead end, in the development of an explanatory system; it has little prestige and should be purely heuristic. The typical (and usually sole) example is behavior on a test correlated with (or used to predict) some other behavior of interest (performance in school, psychotic behavior, etc.).

IN FAVOR OF THE S-R FORMULA

The preference for the restriction of the IV's to stimulating or environmental conditions is understandable from several points of view. First, it opens the way, at least in principle, for the development of a general system to predict and control all behavior. The stimulating conditions can generally be brought under experimental control, or at least descriptively specified. It follows that with appropriate S-R

laws at hand, behavior can also be controlled or at least predicted. Even if some behavior can only be predicted from other behavior [$R_1=f(R_2)$], it should be possible to predict the latter in turn from stimulating conditions [$R_2=f(S)$]. Conversely, the R-R formula does not give the psychologist as much power. R_1 can be controlled only if R_2 is controlled. But if $R_2=f(R_3)$, we must first control R_3. And so on. At some point in this regression, behavior must be predictable from stimulating conditions if it is to be controlled, and the S-R principle insures that this necessary condition is met.

A second reason in favor of the S-R formula is that such a law is more likely than the R-R formula to approximate the description of a causal relationship between the two variables. I do not want to enter the complex problems of causality, but an elementary distinction seems clear enough: Let us say that response rate (R) is controlled in part by hours of deprivation (S). The causal connection here seems legitimate. But (as Kimble points out) if we can predict feeble-minded behavior (R_1) from a test score (R_2), it is equally clear that R_2 did not *cause* R_1 in any sense. Thus R_2 serves exclusively as a predictor. Again the S-R formula seems to provide the more powerful type of analysis.

A third argument in favor of the S-R formula concerns the construction of theories. Its exclusive use makes it possible to define (or "anchor") the intervening variables purely in terms of stimulating conditions. Examples of this are given by Bergmann and Spence (1941) and Spence (1944) in their discussions of the intervening variables in Hull's system. They show, for example, that a construct like stimulus trace is actually defined exclusively by stimulus variables of intensity, time, and the like. These writers contrast Hull's work with Lewin's, where constructs are defined partly by reference to R-variables, or, what is worse, by the "phenomenological report" of the observer. And according to them, such naivete should certainly yield to a system like Hull's, where, when all the intervening variables are reduced to their defining terms, the dependent variables can in turn be expressed as a function of environmental variables. The prediction of behavior takes this form, and the actual R-values can then be compared with the predicted ones.

LIMITATIONS OF THE S-R FORMULA

The appeal of the S-R formula is strong. It points to an elegant and parsimonious ideal for the development of psychological theory. But does it lead to such a development also? There are a number of reasons why it cannot. Furthermore, the R-R formula is much more useful than the restricted examples offered by S-R theorists would indicate. In what follows, the symbols Ri and Rd will be used to distinguish between the use of R-variables as independent and dependent respectively. S-variables are always independent, of course.

The first and most general objection is that the S-R ideal is, as

a matter of brute fact, hardly ever attained. Our psychological laws are, overtly or convertly, not in the S-R form. A better description would be R=f(S,R) to indicate that stimulating conditions will only in conjunction with (previous) behavior be sufficient to predict the behavior under study. The Ri variables may enter into the law in several different ways. For the sake of brevity and clarity a fictitious but plausible example may be cited, rather than an array of cases from the literature.[1]

A psychologist determines the variables governing the number of responses to extinction. Investigation with different ratio schedules leads to the following empirical law: For a given number of reinforcements, the number of responses to extinction is directly related to the ratio of unreinforced to reinforced responses in linear fashion, or

$$R_E = a\frac{R_U}{R_R}$$

Ri variables enter this question in a central manner. This is not a case of the "typical" R-R law referred to earlier. Here, the behavior enters directly into the determination of environmental conditions; it provides the contingencies on a ratio schedule on which reinforcement depends. The behavior is then used to define the ratio that is the principal feature of the law itself.

The S-R theorist could now question whether it is really necessary to state this relationship in terms of R-variables; could they not be replaced? The critical feature, after all, seems to be the difference in reinforcement contingencies, and these are S-variables. Ri variables can simply be eliminated by substituting trials for responses. The equation becomes

$$R_E = a\frac{T_U}{T_R}$$

Perhaps this indicates one reason why so much of S-R theory is based on discrete trial learning: One need not specify the response in describing the conditions of a trial. This solution, however, implies that the observation of behavior on the learning trials is not relevant for the prediction of the behavior on extinction trials.

Such a restriction seems neither sensible nor feasible. Do we not *presume* that there is a response on every trial, whether observed or not? What happens if there is not, or if the subject does not consume the reinforcements? The function will certainly change. In other words, the occurrence of responses and the consumption of reinforcements

[1] The example is not entirely fictitious. Keller and Schoenfeld (1950, p.88f) report a study like this from their own laboratories. They used an interval reinforcement schedule, and individual differences in responding provided differing ratios of unreinforced to reinforced responses. In the present example this ratio would be controlled experimentally through a ratio schedule.

become essential or boundary conditions to the application of this formula where Ri variables are supposedly eliminated. Certainly observation of the behavior is necessary to see whether these conditions are met. In fact, the completion of a trial is usually specified in terms of the occurrence of behavior. Eliminating the Ri variables from the *formula* expressing the relationship incorporated in a law does not eliminate them from a *complete statement* of the law.

A further disadvantage in the substitution of trials for responses is that it eliminates operant behavior or any other method that does not use discrete trials as the method of study. Considering the vast contribution that the operant techniques have made in the last 25 years, this consequence is not to be taken lightly.

A third objection to the elimination of Ri variables is that this may hide the events that are causally of critical importance. Formally the law in its second form may be sufficient to predict the responses of interest. But if we are to ask why this function has its particular form, we will look to the occurrences of behavior as the critical events, not to the presentation of trials. As pointed out above, there are cases of the "typical" R-R law where R_2 refers to a test score and gives no insight to the causal determination of R_1, but this does not mean that every R-R law suffers from this inadequacy. Conversely, an S-R law where all reference to Ri-variables is eliminated will not point to the complete set of events that are involved in the causal determinations of the dependent variables.

A fourth observation refers to the nasty parameters that abound in S-R formulae. (Here, only one was given, namely *a*.) Many S-R functions allow the theorist to say what kind of a relationship relates R to S, such as parabolic, exponential, etc., but the exact function is rarely specified. The parameters presumably are determined by the conditions of the experiment and the nature of the subjects and must be empirically specified. If the values of the parameters are due to such factors as the behavioral history of the subjects, individual differences, and the like, they certainly represent a number of R-variables that are not specified. Here again, Ri variables hide under the guise of S-variables.

ADVANTAGES OF THE R-R FORMULA

Ri variables are not only unavoidable in the complete statement of psychological laws, but they are necessary and valuable adjuncts in the development of scientific psychology. Several instances can be cited. First, individual differences may be put to good use. These are usually considered a nuisance: Under identical S-conditions, different subjects will exhibit different values of an R-variable. If the prediction of behavior is limited to S-variables, these differences remain largely irreducible. Only large sampling provides the technique for equating them among different experimental groups. But if we can find a

second R-variable that is related to the one under study, the effect of individual differences can be reduced by matching groups after a pre-test which is administered to all subjects under the same conditions. Or, the reduction can be made statistically if random assignment to groups is desirable. Better yet is the design where all subjects can be given more than one experimental condition, so that individual differences are held constant between conditions. This design implicitly uses an R-R principle, that response levels between different stimulus conditions will be correlated for the subjects.

Perhaps the use of R-R relationships to exploit individual differences appears purely heuristic, providing a refinement in experimental control within the framework of S-R relationships. There are cases, however, where the investigation of R-R relationships can be of direct value in the study of psychological processes, particularly where the S-R function is only partly known. If extinction is best described as the association of new responses to stimuli which, under conditions of reinforcement, have elicited the conditioned response, we would expect the acquisition of these new responses to be governed by the same variables as the original learning. Subjects that condition rapidly should extinguish rapidly. The study of this R-R relationship will therefore delineate the processes involved. It has been found that the correlation is negative rather than positive, leading to the conclusion that conditioning and extinction are different processes (cf. Hilgard and Marquis, 1940, p.118f). To take another example, we may be intrigued by the nature of the reinforcement provided by cranial stimulation. This could be investigated by the observation of activity that is correlated in strength with the behavior leading to cranial stimulation. If we find that mating activity is high when bar pressing for cranial stimulation in a certain area is most rapid, this will show that these different kinds of behavior are under control of the same independent variables—perhaps androgen level in the blood.

Generalization gradients can be obtained from pigeons after training them to peck at a monochromatic light of a given value, when the wave length of the light is changed (Guttman and Kalish, 1956). The exact form of the gradient cannot be predicted in advance for different subjects or different experimental situations. But it has been found that if the choice behavior is investigated between two stimuli lying on the gradient while the gradient is also obtained, the direction and degree of preference can be predicted from the gradient, (Honig, 1958). This indicates that behavior in a complex situation is controlled by the independent response strength to "simple" stimuli comprising the situation. This conclusion is based on an R-R relationship and does not depend on any particular S-R prediction.

In the above examples the precise S-R relationship may be unknown and difficult to determine. But in each case, the determination of an R-R relationship furthers the analysis and understanding of the behavior

involved. Speaking in more general terms, the R-R relationship is particularly of value in the following situation: The S-variables controlling R_1 are not known, but a law can be established between R_1 and R_2. Then, if the S-variables controlling R_2 are known, or can be discovered, this provides a strong clue to the S-variables controlling R_1.

Again, such an exploitation of R-R relationships may be looked on as an adjunct to what is basically an S-R analysis. But there are instances at hand where the R-R function actually reflects the effect one response on another, and where such effects are used in theoretical analysis. Empirical examples are in the non-randomness of guessing (Skinner, 1942), by rats' tendency to alternate turns in the maze, (cf. Hilgard, 1951), in the classic studies on satiation (Karsten, 1928), and in many psychophysical studies (e.g. Verplanck, Collier, and Cotton, 1952; Senders and Sowards, 1952; Goldiamond, 1955). I am not sure that a "response effect" has been isolated and analyzed in statistical learning studies, but it is clear that subjects tend to avoid giving the same response in an extended series of trials, even when the probabilities associated with each trial are independent (see Jarvik, 1951). In the study of imprinting it has been found that one variable controlling the strength of the imprinting is the effortfullness of the first responses of following the "mother" object (Hess, 1958).

The experimental orientation of this paper does not do justice to the relevance of R-R relationships to genetic psychology and personality. Indeed, personality is often defined as a totality of R-R consistencies. Fixation on the heredity-environment question may lead many psychologists to think that the determination of development and personality is exclusively the province of these two factors. But the fact that behavior at one time will be a determinant of future behavior must be recognized, and this sort of a relationship may be easier to isolate than environmental (S-R) and hereditary (O-R) effects.

From theoretical analysis we may cite the motor learning theories of Kimble (1949) and Ammons (1947), who need to assume on the basis of R-R relationships observed in their data, that a motor response builds up a detrimental inhibitory state which must dissipate before it can regain full strength. In another case, an R-R relationship has been advanced in a theoretical explanation of a supposedly S-R phenomenon. Wyckoff, Sidowski, and Chambliss (1958) suggest that the secondary reinforcing power of stimuli associated with primary reinforcement is artifactual. What may happen instead is this: Stimuli initially associated with primary reinforcement become cues for the "goal-directed" behavior by which primary reinforcement is obtained. When these stimuli are made contingent on a previously unreinforced response, their occurrence raises the general activity level (by initiating the "goal-directed" behavior), and the frequency of the "new" response increases. Here the R-R relationship is between the "goal-directed activity" and the "new" response. Finally, the concept of chaining

in its dependence on response-produced stimuli is familiar enough. The occurrence of R_1 produces cues that are sufficient to elicit R_2, which in turn produces the cues for R_3, and so on (see Keller and Schoenfeld, 1950).

It may be argued, of course, that in all these cases, one response does not immediately affect another, but rather that the stimuli produced by the first influence the second. This is not relevant to the present discussion. The response-produced stimuli are not observed or manipulated; they are not part of the environmental conditions as that term is usually understood; they have the nature of explanatory constructs. The true independent variables in the cases quoted above remain behavioral variables. It would serve the interests of parsimony for theorists to recognize this, and to incorporate R-R relationships openly in their systems, rather than try to fit the facts into an S-R mold by the postulation of hypothetical stimuli or central states. The domination of the S-R concept leads to the "multiplication of entities beyond necessity."

BEHAVIORAL CRITERIA FOR PSYCHOLOGICAL CONCEPTS

The observation of behavior enters psychology other than as a dependent variable in one further, and very essential fashion. It is a criterion for many (if not most) of our important concepts, even when they refer to stimulating conditions. We need not be surprised that concepts based explicitly on the observation of behavior have this characteristic, like *reminiscence, rage* or *disinhibition;* we expect hypothetical constructs like *hunger, anxiety,* or *reactive inhibition* to be "anchored" to behavioral as well as environmental variables; but how about terms like *reinforcement, deprivation, equivalent stimuli, frustration,* or *punishment?* Any sensible definition of these, though ostensibly referring to stimulus events or conditions, must include behavioral criteria to indicate when they can be brought under the rubic of the concept being defined. Reinforcement, in Skinnerian terms, is clearly put on a behavioral basis, since the criterion for a reinforcing stimulus is that a change in response rate follows its systematic introduction as an event contingent on the response. In the case of deprivation the behavioral component is less obvious but equally essential. Does it make sense to talk about a non-addict as drug *deprived,* or a cow *deprived* of cultural influences? Is a man *frustrated* by a *barrier* that hinders the progress only of an infant? Can a *goal* be determined independently of behavioral observation? And so on. In all these examples, a behavioral criterion must be used to determine what kinds of environmental circumstances control behavior before we can fruitfully work with them as independent variables.

It is interesting that Kimble (1953) uses frustration as an S-condition in an example where he contrasts S-R and R-R relationships:

> There is some rather good evidence to suggest that an

> individual who is frustrated in his various strivings develops a tendency toward aggressive behavior. For purposes of exposition, it can be assumed that this is a well-established relationship. On this basis, if one knew that an individual had been frustrated many times in his efforts to achieve important goals, it could be predicted that he would show aggressive behavior. In this case, an S-R law would be used to make the prediction. The determining S-variable is the frequent blocking of goal-directed behavior, and the dependent response is the aggressive behavior (p. 48).

Note that the "determining S-variable is the frequent blocking of goal-directed behavior." Certainly the identification of these circumstances demands the observation of some R-variables; how else can "goal-directed behavior" be identified? The R-variables are therefore incorporated in the "determining S-variable" by definition. It seems that again we arrive at the more complete formula: $R=f(S,R)$. To indicate that here it is the *definition* of the S that depends on the conjunction of an R-variable, a happier expression would be $R=f(S_R)$. Since in the normal case, R-variables enter into the prediction of behavior both in the definition of S-terms and as independent empirical factors, the most complete formula is $R=f(S_R,R)$.[2]

In closing, we may point out that the behavioral criteria bind different instances of terms like the above together and indicate that we should seek out their systematic importance. The physical aspects of a barrier between a child and his toy, a rat and her pups, and a professor and his promotion, are too different to permit a connection, but on the basis of some communality from a behavioral point of view, we call these by the same name. Any generalization from, say, the behavioral effect of the rat's barrier, on the basis of *physical* aspects of the environment, would be meager indeed; we seek to generalize better on a basis of *behavioral* similarity as it is used in the definition of *barrier*. We are reminded of Kantor's "stimulus function" (as distinct from stimulus per se) (cf. Kantor, 1938) and of Koffka's "behavioral environment" (as distinct from the physical environment) (cf. Koffka, 1935), terms that stress the behavior-related characteristics of stimulus identification and classification. In psychometrics, the importance of making scales that are defined in terms of behavioral criteria is increasingly recognized, since physical dimensions are often not relevant to the analysis of behavior, or else cannot be isolated (as in an attitude scale). For a fruitful investigation of the laws that are the major substance of psychology, the behavioral criteria for the definition of the independent variables should be

2. Behaviors are often partly defined with reference to stimulating conditions; therefore R should have a subscript S as well as the reverse.

openly used, for only then will the features of the environment that are critical for the determination of behavior be identified.

SUMMARY

The traditional S-R position that in systematic psychology the IV's are to be identified with environmental conditions is critically discussed. The advantages of this position are mentioned in that it points to a complete and parsimonious system for the prediction and control of behavior. Arguments against the position are then presented. First, behavorial variables cannot be omitted from the complete statement of the independent variables and boundary conditions of a psychological law. Second, behavioral variables can serve usefully as IV's both as adjuncts within an S-R framework and in the investigation of R-R relationships in their own right. Finally, the point is stressed that behavioral criteria enter into the definition of most psychological concepts even when the concept ostensibly refers only to environmental circumstances. It is these criteria that lend systematic importance to the concepts in psychology.

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The Psychological Record, 1959, 9, 131-142.

EVOLUTION AND THE SCIENCE OF PSYCHOLOGY

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An occasion such as the present centenary celebration of the publication of Darwin's great work *On the Origin of Species by Means of Natural Selection*, does no more than intensify the stimulation to consider evolutional problems. The nature, direction, and processes of evolution are perennial topics of reflection and discussion. But here one thinks primarily of organic evolution in the sense that Darwin did so much to make popular. Evolution, however, is much more than a feature of the organic domain. It permeates all scientific departments. Students of scientific psychology especially have much to gain from the study of evolution not only as a process in organic nature, but also as a general series of scientific events and principles which have great influence upon psychological facts and theory.

WHAT IS EVOLUTION?

To ascertain the nature of evolution and how it can affect any particular science and especially psychology, we must make both a semantic and an event analysis. Obviously the term "evolution" stands for many things. We must not only distinguish between a) authentic events and b) the ideas or reactions to them, but also between both of these and c) putative but nonexistent designata of evolutional terms. Ideas, beliefs, and other reactions to authentic evolutional events we call evolutional constructs, while the non-existent designata we call constructional or verbal products. The acts of constructing such nonexistent designata are, of course, events and we can usually trace out the motives and other conditions for performing such actions. It is clear, however, that even when we ruthlessly dissociate our discussion from the autistic products of such behavior, for example, 1) acts or processes of divine creation, or 2) directive plans or teleological goals, we are still left with a huge problem of identifying and evaluating evolution.

Evolutional Semantics. Among the events symbolized by the term "evolution" are a) general changes in anything in the cosmic system, b) biological transformation, c) the particular origin of species and varieties, and d) the development of social things and institutions, for example, the development of particular nations, societies, laws, etc. "Evolution," of course, stands also for theories, ways of thinking, social and religious attitudes, and various forms of beliefs which imply origin, change, and improvement in things.

Evolutional Events. When we proceed to analyze evolutional events we at once face the following questions.

Is evolution:

a) a process newly discovered in the 18th or 19th century, or is it something that has been observed and studied as early as the Greeks?

b) a series of fortuitous organic changes, or modifications of plants and animals through definite natural selection?

c) a series of general, natural, event-changes, or exclusively the specific modifications of things, including organisms?

That these questions can be asked and variously answered testifies to the facts that evolution is something more than a biological topic, and that it requires considerable analysis and interpretation.

As to the first point it is clear that evolution as an event or series of events operates in many fields. Perhaps Zeller (1878) and Osborn (1929) are correct in tracing a continuity in evolutional thinking from the earliest Greek cosmologists to the Lamarckian and Darwinian theories.

Be that as it may, it is incumbent upon us to determine whether we are dealing with a) data, b) method of observation or study, or c) theory or interpretation. Also we shall have to decide whether we are concerned with indefinite general processes or whether we are examining very definite, specific occurrences. But first we must elucidate the distinctions between evolutional data, methods, and interpretations.

EVOLUTION AS DATA, METHODS, AND INTERPRETATIONS

Granted that the topic of evolution widely pervades all scientific domains this is certainly in part accounted for by the three-fold role of evolution in science. Evolution is a datum or a subject-matter in science, a method or investigative tool, and finally a mode of interpretation.

Evolutional Data. By data we understand events which present problems for investigation. Among evolutional events may be numbered the collocation of things and circumstances which gave rise to the solar system or any astronomical unit such as a particular galaxy or system of galaxies. The great multitude of astronomical objects which in greater or lesser measure influence each other force upon the astronomer the evolutional problems of origin and development. His recurrent theme is the origin of nebulae and their transformation into stars. Since the discovery of radioactivity and the development of atomic physics with their intimate and inevitable processes of change and transformation physicists have been made into evolutionists. Similarly the chemist who is obliged to account for the development of the simple into more complex compounds, and the origin and development of all chemicals from a primeval hydrogen base has also become engrossed with evolutional problems. We hardly need to mention the biologist's perennial preoccupation with evolutional processes.

with the origin and modification of types of animals and the more localized development of an organism from a fertilized cell, and with many other comparable happenings. Finally, as we shall see, the entire science of psychology may be looked upon as the study of the evolution of various types of behavior.

Evolution as Investigative Method. Because of the pervasive changes and transformations in the events studied by all the sciences it was inevitable that a distinctive historical or genetic method should arise and take on various specific characteristics depending on the particular investigative field. Examples of the genetic or historical method are a) the pushing back of observed current changes to earlier stages in an effort to understand previous evolutions, b) the understanding of current conditions in biomorphology and function by the observation of prior transformations, and c) the devising of models or analogies to demonstrate how evolutional processes occur in nature or in the investigative situation, both in the field and the laboratory. An excellent example of investigative operations in the domain of biological evolution is the series of operations designed to produce mutations which change the direction of organic development.

Evolutional Interpretation. The domain of evolutional interpretation covers the enormous families of assumptions, beliefs, and theory constructions concerning the existence of evolutional processes, their modes of operation, and their productive direction. Samples of such constructions are a) the broad philosophical schemata, b) the theological belief in the divine origin of transformation, c) the host of theories called "orthogenesis," "aristogenesis," "hologenesis," "nomogenesis," "telefinalism," and finally d) the more specific hypotheses such as the random or non-random genetic determination of change and development.

In view of the fact that events or data, methods, and interpretations are inevitably interrelated, they affect each other. The ideal situation is obviously one in which the methods and interpretations are fashioned with close reference to the events. But because events are complex, prejudices powerful, and vested intellectual interests dominating, there are often deep and uncrossable gaps between them.

It is not to be overlooked, however, that the best way to employ evolutional principles is to stress their natural, empirical, and specific character as over against broad general speculations. Even when evolutionary interpretations are contaminated with first causes, with initiative design, the primary feature remains the observation of concretely occurring interactions of factors in particular situations. When evolutionary interpretations are confined to estimations and interrelations of events they are heavily weighted in favor of specific happenings and component details.

THE SCIENTIFIC IMPORTANCE OF EVOLUTIONAL METHODS

When we peel off all the layers of assumptions and other constructs from the nucleus of evolutional data or events we find essentially processes of change and development. For this reason evolution plays a part in every science. In a broad doctrinal sense the postulate of universal change and transformation corresponds to the postulate of the conservation of matter and energy. In this sense organic evolution covers a special type of events and a special mode of investigative procedure.

But inasmuch as changes and transformations in events display an enormous variety, the part that evolution plays in the various special sciences varies likewise. In some sciences, for example, in astronomy and physics, the evolutional emphasis is more on origins than on current changes. In biology, by contrast, the interest in origins is equally matched with that in current processes. When we compare biology with psychology we find that in the former the evolutional changes and transformations are interconnected with an entity or substantive form, that is an organism, while in psychology the evolutional processes are connected only with acts. In biology current behavioral changes are functions of morphological or anatomical structure. In psychological activities the organism performing the activity is not structurally as important as the mutual interactions it performs in connection with other things. The psychological event is essentially an operational field.

When we sum up and analytically identify the role of evolution in science we invariably find emphasized its investigative function. We indicate four characteristics of the evolutional or the historico-genetic method.

Stress of Events. When we concentrate on change and transformation we are bound to make our postulates and theorems, in short our constructions, conform to instead of dominate the events studied. We must ask whether the changes observed give us any basis for asserting that the development is going in any particular direction. All that we can do is to observe the interrelation of the factors in the changing field. The observed processes may be reversible or irreversible. At any period the original combination of factors may comprise different configurations. This point may be illustrated by the general problem of entropy. Physicists may assume that entropy is constantly increasing and the universe tending toward an inevitable asymptote of ultimate stagnation. But this view may be illusory. When any observed degradation of energy is studied from the probability standpoint we may discover that the downward curve really consists of self perpetuating fluctuations (Lotka, 1924). Obviously we stand here on the borderline between the observed events and the interpretive construction. Evolutional principles support the scientific wisdom of cleaving to observed changes, and not seeking such unwarranted extrapolations as final goals. It is only the problems of ultimate origins that bring the scientists to the undesirable merger of events and constructs. It must

be emphasized that for the most part evolutional events are observed in *medias res* and thus origins are arbitrarily selected. When authentic origins are sought they are only to be found within the great complex of developmental changes of all sorts. But even these problems or origins belong to highly specialized inquiries. Biologists and psychologists are in the fortunate situation of dealing with fairly gross changes.

Concrete Data. Another investigative trend fostered by evolutional method is that toward the minimization of abstractions. It emphasizes the qualitative aspects of events as well as the quantitative and relational ones. Evolutional principles encourage the questions of what changes are occurring when rates, intervals, and ratios are observed and recorded. At the same time the evolutional way of thinking exposes the fallacy of setting in opposition the genetic-historical and the abstractive hypothetico-deductive methods. This fallacy conceals the fact that so-called rational methods overstress the assumptional phase of scientific work.

Specificity of Events. Still another investigative trend of evolutional methods is the high value placed upon specificities. The genetic-historical method promotes the search for the details of events, the particular links in event chains, and the components of event fields. Generalities like averages, massed performances, and other more or less remote constructions cannot be substituted for particular complexes of factors. Constructions are equally kept under control, inasmuch as the proportion of autistic and independent creation to description and measurement is carefully controlled.

Field Structure. And finally we may point out the tendency of the historico-genetic method to stress situations, fields, and factor complexes instead of isolated entities. Accordingly the scientist using the evolutional method searches for interrelations and interactions of few or many specific factors. He deals with atoms and elements in interaction under specific conditions, with organisms and their environments, with psychological fields of action, and similar constellations.

At the conclusion of this analysis of the role of evolutional thinking in science two important points must be made clear. In the first place the primary argument that is made for the historico-genetic approach is that its use and results are based on the facts, that it follows the character of events and is not arbitrarily set up. In the second place and as a consequence of the first, this method does not compete with any other. It is in fact a trait of the method that it comports with any abstractive, mensurative, and quantitative operations required for the ascertainment of the qualitative, quantitative and relational attributes of event fields.

THE ROLE OF EVOLUTION IN PSYCHOLOGY

Now that the nature and merits of the historico-genetic method have been delineated we inquire what particular influences evolution

has exerted upon psychology. Obviously since psychology is so close a cognate of biology we should expect it to profit greatly from the development and improvement of evolutional thinking and the practice of observing historical processes. The impact of evolution upon psychology has not only resulted in a general revolution of psychological theory and practice, but also has shaped its detailed development. Evolution has moulded psychology by means of its two phases, a) the special Darwinian organic evolution of the 19th century, and b) the general genetic and historical methods. Darwinian evolution has done much to free psychology from the stultifying adherence to some variety of immortal-soul doctrine. The general historical and genetic methods of evolution have influenced psychology to search for specific concrete factors to account for psychological events and to concern itself with observable behavior as its subject matter. It is undeniable that evolutional thinking introduced into psychology a viewpoint and investigative practice which has made it into a naturalistic and objective science. This happened despite the fact that such outstanding biological evolutionists as Darwin, Spencer, Galton, Huxley and others were ingrained mentalists. What Darwin, for example, hoped to demonstrate was that the transcendent mind evolved in a way similar to that of the body (Darwin, 1871; Darwin, 1872).

Concerning evolutional thinking in psychology one may repeat what has been said of relativity thinking in physics, namely, that it upset an axiom. In psychology the displaced axiom was the proposition that psychology dealt with powers or processes which determine and control the behavior of organisms. The new axiom or postulate that replaced it made plain that it was behavior itself that constituted psychological events (Kantor, 1935). This axiom-replacement consisted of an evolutional process itself. In its earliest stages the axiom merely held that the mind, which was retained from older stages of psychological traditions, was connected in some unknown way with the structures and biological functions of organisms. Later, came an increasing realization that the mental processes were intimately connected with behavior. This we may regard as the ecological or functional stage of this evolution. And finally psychology attained the view that there is really nothing else in a psychological situation but various kinds of behavior of organisms and their stimulating objects.

EVOLUTIONAL DOCTRINE AND THE EVOLUTION OF SCIENTIFIC PSYCHOLOGY

To demonstrate the specifc way in which evolutional doctrine helped to channel psychology in the direction of natural science we may compare its efficacy in this role with four other movements that also contributed to this result.

Psychophysiology. When students of psychological events became influenced by the advancing conquests of science in the 17th century they made brave attempts to secure for the psychic processes the bene-

fits of contacts with actual events. From this arose the view that by positing a connection between the mind and the body, the soul or mind would achieve a spatial locus, no matter how tenuous. Psychophysiology proliferated with Cartesian interactionism, Liebnizian parallelism, and Spinozistic double aspectism. Psychological history has passed its just sentence upon such futile and sterile attempts to naturalize the soul. Still the stress on body was a historical step forward.

Materialism. The historical dissatisfaction with psychophysiology brought in its train the notion that the mental is not a primary substance or process but something dependent upon and inferior to the material or nonmental factors of inorganic and biological things and events. The important biological things were the machines constituting the bodies of both human and animal beings. Even if it is true that historical materialism has had some influence in developing behaviorism and objective psychology, its primary merit is that of demonstrating that even a false doctrine can sometimes lead to a modicum of progress.

Quantization. A distinctive stage in the evolution of scientific psychology is marked by the attempt to profit by the quantitative methods that astronomers and physicists adopted to achieve their successes. But where lies the value of quantitative methods? Here the historico-genetic approach supplies the answer. Neither the psychological nor the physico-astronomical sciences owe their successes to the slogan that the book of nature is written in the language of numbers. On the contrary, quantitative procedures are scientifically effective because all significant numbers are derived from measurements made upon the interactions of observed things and events.

Experimentation. Because experimentation is *ipso facto* one of the most effective means for scientists to get into intimate contact with things and events, experimentation became an inevitable stage in psychological evolution. But since manipulation and measurement depend upon specific hypotheses and general postulation, the results attained are no better than the assumptions which regulate the procedure. The value of experimentation in the development of psychological science rests, then, upon the principle of contact of workers with things worked upon. If the psychologist assumes that he is working with impalpable and ineluctable psychic states that are manifested in the observed actions of organisms, the data he obtains will be murky and in need of analysis and refinement. Fechner and Wundt did not contribute to psychological development by any analogous application of experimental methods to psychic research, but by bringing subjects and their reactions to stimuli into the investigative domain of psychology.

Evolution. In many ways the impact of evolutional methods upon psychological progress was much more viable and successful than any of the others employed throughout the history of psychology. Not only do evolutional methods dictate strict preoccupation with occurring events but in addition they force attention to details of happenings.

Since changes and transformations are dependent upon interrelations of factors in specific situations, the genetic or historical method is less liable to be infected with improper assumptions than other methods.

PSYCHOLOGY: AN EVOLUTIONAL SCIENCE

So far we have considered the effects which evolutional doctrine has had upon psychology as a science. The changes in psychology we may regard as institutional, that is they concern psychology as a particular formal discipline. But the connection of psychology with evolution goes deeper. For one thing evolutional doctrine has been influential in the development of several departments of psychological investigation. Beside this stands the fact that psychological events themselves are invariably and fundamentally evolutional. We consider these evolutional aspects of psychology in order.

A. EVOLUTIONAL SPECIALIZATION IN PSYCHOLOGY

When Darwinian evolutionism demonstrated the continuity pervading all organisms and the changes which specific conditions helped to effect in form, function, and behavior, psychology developed the following series of specialized studies marking great psychological progress.

Genetic Psychology. From evolutional developments psychologists have learned the great value of studying the actions of organisms as they pass through their individual developments towards maturity and then proceed down the curve leading to extinction. The general notion of evolution or genesis of action gave rise to the specialized study of the inception and performance of action in infancy, childhood, adolescence, maturity, and advancing age.

Animal and Comparative Psychology. Consequent upon the breaking down of the barrier between the human and the nonhuman animals, which evolutional doctrine achieved, psychologists were encouraged to study the reactions to stimulus objects of many kinds of organisms. Darwin himself stressed the psychological continuity between human and nonhuman animals in his book *Expression of the Emotions in Man and Animals.* This new attitude not only extended the range of psychological study but also led to many useful behavior comparisons.

Behaviorism. The successful study of animal behavior and the comparisons made between the activities performed by different species of organisms led more or less directly to the assumption that an internal force or function called variously soul, mind, or consciousness was entirely unnecessary. From this developed the theory that psychology deals only with behavior. In this way psychology was well started on the natural science path. Unfortunately this progress was blocked by transferring to the brain, which had earlier been regarded as only the seat of the soul, the forces and powers of the soul itself. Consequently, the age-old theory was retained that the psychological act is a creative one and that stimulus objects are projected from the inner

resources of the organism's brain. Nevertheless, the accent upon behavior loosened the hold that the soul had had on psychological thinking since the early Church Fathers.

B. PSYCHOLOGICAL EVENTS ESSENTIALLY EVOLUTIONAL

Probably the highest peak attained in the relations between evolutional thinking and psychology is the realization that psychological events are themselves through and through evolutional. Psychological events are in very essence evolutions with a definite origin and course. Psychological events are momentary and recurring action fields, and depend fundamentally upon the biological and cultural evolution of the organisms and the stimulus objects which participate in interbehavioral fields. From this it follows that psychological theory is likewise evolutional.

EVOLUTION OF PSYCHOLOGICAL EVENTS

The basic characteristic of psychological events is their historical inception, the fact that they arise in the lifetime of individuals out of definite processes and situations. The entire repertoire of behavior which an individual performs has all been built up in contact with particular objects in specific situations. This entire process I have designated the fourth evolution (Kantor, 1959, Ch. 4).

The Fourth Evolution. Briefly this evolution consists of the ***Interbehavioral History*** in which the organism begins and continues to participate in interbehavioral fields on the basis of an exceedingly large number of cumulative variations of interbehavior. Psychological evolution is the process of acquiring behavior characteristics, which can be repeatedly reperformed as long as the original components of the interbehavioral fields are available for recombination, or can be substituted for by similar ones.

Interbehavioral history follows directly upon the third or ontogenetic evolution of the organism. This is the embryological development beginning with the union of the gametes in biological reproduction. Each item in the development of individual organisms counts as a potential base for the Interbehavioral History. For example, the ineffective and paranormal development of the cretin or microcephalic will count as an adverse factor from the standpoint of a "normal" standard.

Embryological developments are in turn based upon the second or phylogenetic evolution of particular species and varieties of organisms. From a psychological standpoint we have in the second evolution a tremendous source of potentialities for psychological interbehavior. Those differences in psychological interbehavior influenced by the phyletic or species traits of organisms originate in the second evolutional stage and interval.

Although it is difficult to illustrate specific psychological potentialities arising from the first or inorganic evolution, which results in the

possibility of organisms developing altogether, there are good reasons for not overlooking this phase of the evolutional continuity.

The Cultural Evolution. Inasmuch as psychologists trace out the details of what are inevitably evolutions of interbehavioral fields, they must take into account the fact that such fields are not exclusively dependent upon the biological potentialities. For one thing, psychological events are interbehavioral; just as the evolution of organisms and their behavior are field events and inevitably involve objects, settings, and interbehavioral conditions, so throughout the fourth evolution the stimulus objects and the other factors such as settings and interbehavioral media must be equally emphasized. Accordingly, for the purposes of human psychology we must take into account the important evolution of cultural institutions with their complement of objects and behavioral entities—customs, social habits, and so on—which influence the development of the complex, detailed psychological actions. One of the merits of close adherence to psychological evolution is the emancipation of psychology from laws and principles derived entirely from the traits of the behavior of such organisms as rats, hamsters, monkeys, apes, etc. The evolutional principle helps the psychologist to take a comprehensive sample of observations as a basis for his laws and systems.

EVOLUTIONAL CONSTRUCTS IN PSYCHOLOGY

Since scientific constructs—postulates, hypotheses, interpretations, laws—are intimately interrelated in a system with the events and investigative rules of a particular science, they will obviously reflect the total scientific situation. The following postulates represent a sample of assumptions which emphasize events or data influenced by the impact of evolutional principles upon psychology.

A. Psychology is not concerned with any kind of stuff or entity but only with actual interbehaviors. These interbehaviors constitute complex constellations or fields of factors which may or may not be recombined. When they can be, behavior is replicable though never in any absolute sense. Variability is of the greatest probability.

B. Psychological events are intimately related to biological and cultural events though never reducible to either. The practice of meticulous analysis and description of events not only reveals their intimate traits but also shows their interrelations.

C. Psychology does not deal with powers, forces, or abstract potentialities, whether regarded as innate and internal to the organism or as external to the interacting organism. Among the external forces or principles that are eschewed are all ends or teleological principles. From the genetic and historical standpoint psychological science never needs to go beyond the limits set by the interbehavior of particular organisms with stimulus objects in a framework of concrete conditions.

More specifically, evolutional influences on psychology free it from

such constructions as instincts, drives, internal mechanisms, native intelligence, abilities, and capacities, not only because all these generally designate nonexistent powers and forces but even more because they turn students away from the observation of detailed and specific happenings.

SUMMARY

The centenary of the publication of Darwin's celebrated book provides an excellent occasion for reexamining what evolution is, and what it signifies for science in general and the science of psychology in particular.

Because the term "evolution" stands for two kinds of referents, a) evolutional changes and b) scientific reactions to these changes, I have attempted to clarify the triangle by a brief semantic analysis of "evolution" and by differentiating between various kinds of evolutional events.

Further to clarify the evolutional situation and to distinguish between constructions about events studied and those events themselves, I have differentiated from each other the three roles evolution plays in science. It is necessary to separate 1) evolution as changes and transformations in the events or data studied, from 2) evolutional types of investigative operations, and from 3) evolution as a mode of scientific interpretation.

Our various analyses of evolutional constructs and data have revealed a general scientific investigative principle or doctrine which is derived from the observation of interrelated changes and transformations in event systems. Because of this derivation the historico-genetic principle is extremely important as a guide in scientific investigation. Its merits stem from the facts that, a) it stresses events, b) deals with data as concrete happenings, c) emphasizes the specificity of events, and also, d) the field of organizations in which they occur.

Within the psychological domain the evolutional principle has had basic and wide-reaching theoretical and investigative influences. First and foremost, in theory the evolutional doctrine has forced the replacement of the axiom that mental processes govern psychological action by the axiom that the psychological events consist exclusively of fields of observable mutual interactions of organisms and stimulus objects under specific conditions. Extremely important, too, has been the historical fact that the evolutional way of thinking has helped to set psychology on the naturalistic path more than psychophysiology, materialism, quantification, and even experimentation.

From the investigative standpoint the impact of evolutional principles on psychology has stimulated the development of such new branches of research as Genetic psychology, Animal and Comparative psychology, and the Behavioristic movement. Surpassing this, however, evolution doctrine has forced the recognition that psychological events are essent-

ially developmental processes, and that psychological constructs should, therefore, demonstrate their derivation from such evolutional situations.

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THE EFFECTS OF PRIOR STIMULATION UPON CONTOUR PERCEPTION OF MOVING STIMULI[1]

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There are relatively few visual activities which do not involve contour perception; and yet, when compared with such things as acuity, form perception, and contrast phenomena, the matter of contour perception has received very little attention. In order to come eventually to an understanding of some of the relatively simple phenomena of visual perception, it will probably be necessary first to attend to the manner in which the visual system constructs and maintains an "edge" out of varying patterns of light energy falling upon the visual receptors.

The view of the retina as a mosaic with insulated and independent receptor-projection systems does not seem to meet adequately data relating to the limits of visual resolution. Recent years have witnessed the growth of a model of visual activity based upon statistical concepts (Marshall and Talbot, 1942), and although not without its critics (K. R. Smith, 1952; Deutsch, 1956), the statistical theory has pointed out the possibility of applying statistical concepts to the formulation of theoretical models of visual activity. For example, several of the basic concepts stated in the original work of Marshall and Talbot have been extended to an analysis of figural after-effects (Osgood and Heyer, 1952), form perception (Day, 1956, 1957), and apparent movement (Osgood, 1953).

It is within the framework of the statistical theory that the present experiment was formulated. The phenomenon upon which observations in this study were based may be described as follows: A small stimulus (*A*) was observed moving horizontally through some distance (*D*), from left to right. Its velocity (*V*) was increased until the contours of *A* could no longer be seen. At this velocity, if *A* were exposed briefly in a stationary position prior to movement, then *A* was seen *during subsequent movement* with clearly defined contours.

Data from several experiments (Smith and Gulick, 1956, 1957) indicate that the perception of the contour of a moving stimulus is influenced to an important degree by the visual events preceding stimulus movement. The edges of a small stimulus object which were not seen clearly at a particular velocity *could be* observed at that velocity

1. This research was supported by a grant from the Faculty Committee on Research, University of Delaware. Portions of this paper were delivered at the Eastern Psychological Association's meeting in April, 1959.

provided that the stimulus object was presented in a stationary position before its movement. The duration (T) of the stationary exposure and the maximum velocity (V) at which contour could be resolved during subsequent movement were found to be related linearly. This phenomenon of contour facilitation has been studied under conditions wherein T ranged from 0 to 300 msec and V ranged from 0 to 30° visual angle per sec.

A tentative explanation of these data was offered by Smith and Gulick (1957) within the framework of the statistical model of visual activity. It was suggested that decaying neural activity in the visual cortex following the cessation of the stationary stimulus summed with that activity produced by the moving stimulus. The effect was to maintain the total level of neural excitation at a level sufficiently different from that of the surrounding cortical areas so as to meet the necessary conditions for contour perception. One of the necessary conditions for the facilitation of contour perception during movement was considered to be the summation of neural excitation from the two phases of the stimulation sequence.

If the neural units involved in the excitation produced by the stationary stimulus comprised a totally different population of neurons from those involved in excitation arising from the moving stimulus, then no summation could take place. Accordingly, contour facilitation should not occur. Conversely, maximum facilitation of contour perception should arise when identical neural elements comprised both populations.

The present experiment involved an attempt to manipulate the spatial separations of the distributions of excitation arising from the stationary and moving stimuli. The hypothesis was that separations would have an adverse effect upon contour perception during movement, and that the greater the separation (the less the summation), the more deleterious would the effect be.

METHOD

Observers

Eight trained *O*s between the ages of 17 and 22 years made contour judgments. Each *O* met a visual acuity criterion of 1.0, corrected, and although each was naive about the nature of the present experiment, all *O*s had participated in other experiments involving contour judgments.

Apparatus

A white disc with a diameter of 66 cm was rotated on a horizontal axis such that the plane of the disc was perpendicular to *O*'s line of sight. A white rectangular shield placed in front of the disc in a parallel plane occluded the entire disc except for the portion which was visible to *O* through a small rectangular aperture that subtended a visual angle of 9.0° in width and 0.5° in height. The stationary stimulus was produced by a 90° concentric arc painted in flat black upon the surface of

the disc. The moving stimulus was produced by a 60° eccentric arc. The velocity of the moving stimulus was varied by changing the speed of rotation, and the duration of the stationary stimulus was varied by speed of rotation and/or by occluding part of the 90° concentric arc with a white sector superimposed on the disc. The direction of rotation was counterclockwise: movement was from left to right. Each stimulus subtended a visual angle of 0.5° in height and width, and movement extended through a visual angle of 5°. The stationary stimulus appeared as a square. Due to the "fall" of the eccentric arc, the moving stimulus appeared to *O* as a parallelogram with its right and left edges displaced from the vertical several degrees.

A reduction screen placed midway between the disc and *O* limited the total visual field to a width of 16° visual angle and a height of 5°. An adaptation panel located immediately behind the reduction screen was used both to interrupt *O*'s vision between trials and to maintain a constant level of visual stimulation throughout the experiment. The brightness of this panel and the white shield and disc equalled 70 ft-l, and the brightness of the stimuli equalled 2 ft-l, as measured with a MacBeth illuminometer. The *O* viewed the stimuli binocularly.

A dim pin-point of light was used as a fixation point for *O*. It was always located at the place in the visual field from which movement began.

Five different discs were employed. Each was identical in so far as the eccentric arc was concerned; however, the locations of the concentric arcs differed so that the stationary stimulus appeared at one of five locations in the visual field relative to the place from which movement began. Figure 1 illustrates the important components of the apparatus.

Instructions

The *O* was informed that he would see a small black stimulus appear on the left side of his visual field and that it would move to the right a short distance and then disappear. The *E* directed *O* to report the presence of contour only if the edges of the stimulus appeared clearly defined throughout the extent of movement. The criterion was demonstrated to *O* by showing the stimulus moving at a very low velocity ($V=4°$ visual angle/sec.) During the demonstration the fixation point was noted and *O* was instructed to fixate the pin-point on each presentation *until movement began,* and then to *track* the moving stimulus until it disappeared. Then *O* was shown the same moving stimulus as before but with the addition of the stationary stimulus (duration, $T=100$ msec) preceding movement. Several additional preliminary trials followed in which the stationary stimulus appeared at a location displaced from the fixation point 1 or 2° to the left or right. The *O* was informed that the moving stimulus alone was the one about which contour judgments were to be made, and, in addition, that the stationary stimulus should not distract *O*'s line of sight from the fixation point.

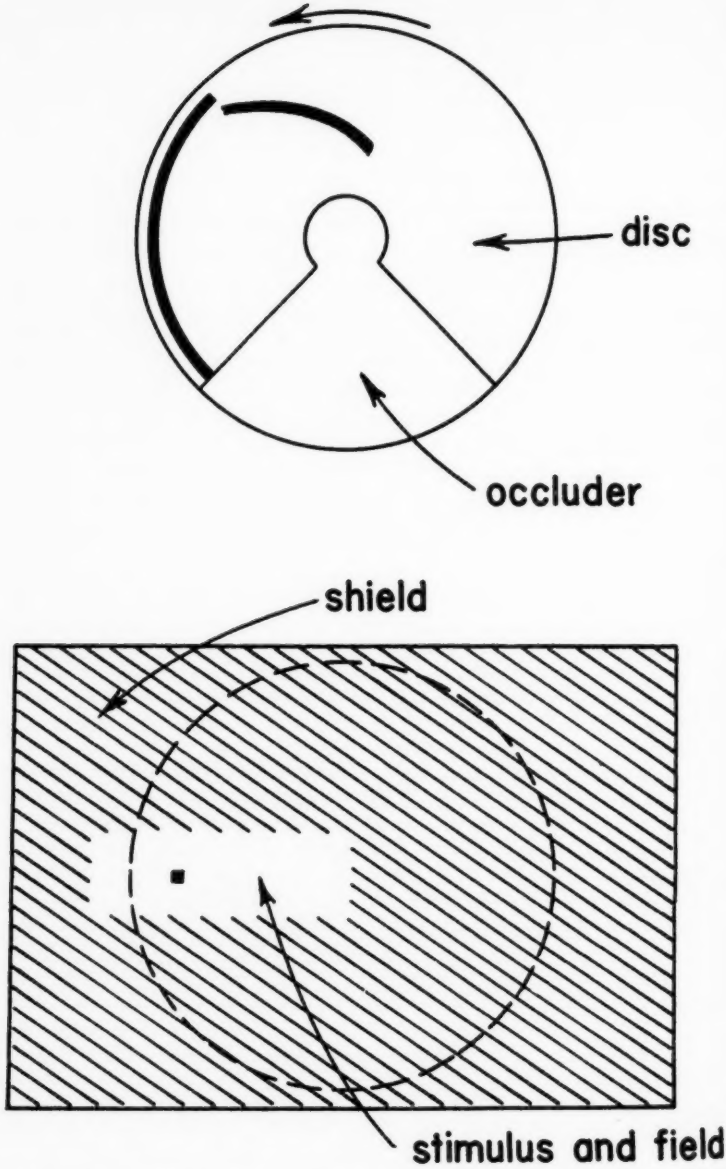

Figure 1. The arrangement of apparatus used to present the stimulation sequence to *O*. This disc illustrates the one used in the condition wherein the stationary stimulus was displaced two degrees to the left.

Procedure

Following the preliminary trials used for purposes of instruction, the moving stimulus was shown to *O* at a low velocity, with $T=0$. Four successive presentations of the stimulus (a trial) were given, after which *O* reported the presence or absence of contour on the last presentation. By following a method of limits, *V* was increased in 1° steps until *O* reported the absence of contour. At this critical velocity, the stationary stimulus was introduced for a brief duration ($T=10$ msec). This duration was increased in 10 msec steps until *O* reported that contour of the moving stimulus had been regained. Thereafter, *V* was increased until the contour of the moving stimulus could no longer be seen. Again, *T* was increased until contour was restored. This procedure was continued until either the limit of *O*'s discrimination or the limit of the apparatus was reached. This method established combinations of stationary stimulus durations and maximum velocities of movement which would allow the perception of contour during movement.

The relationship of *T* to *V* was determined for each of eight *O*'s in a control condition and in each of four experimental conditions. The control condition consisted of having the stationary stimulus appear at the same place in the visual field from which movement began (the fixation point), whereas the four experimental conditions involved displacement of the stationary stimulus from the fixation point measured in degrees of visual angle at 1 and 2° left and 1 and 2° right. The order of experimental conditions was random for each *O*.

RESULTS

Data from this experiment indicate that spatial displacements of the stationary stimulus from the place in the visual field from which movement begins have a deleterious effect upon contour perception during movement. Moreover, the effects of displacement appear to be related directly to the extent of the displacement.

In Figure 2 are presented functions describing the combinations of *T* and *V* which allowed contour perception. Since displacements of equal extent to the right and to the left of the fixation point produced equivalent effects, data were combined across this variable. In the control condition data were obtained with 0° displacement. Each function is based upon data obtained for all *O*s, and although the slopes of the functions varied from individual to individual, there were no instances of overlapping functions: that is, the greater the displacement, the steeper the slope.

The control function demonstrates that the moving stimulus could not be seen clearly contoured at velocities exceeding approximately 9.4° visual angle/sec *unless* the stimulus were presented in a fixed position for a brief interval prior to movement. When $T=160$ msec, the maximum velocity at which contour could be resolved was increased from 9.4 to about 15°. However, displacement of the stationary stimulus

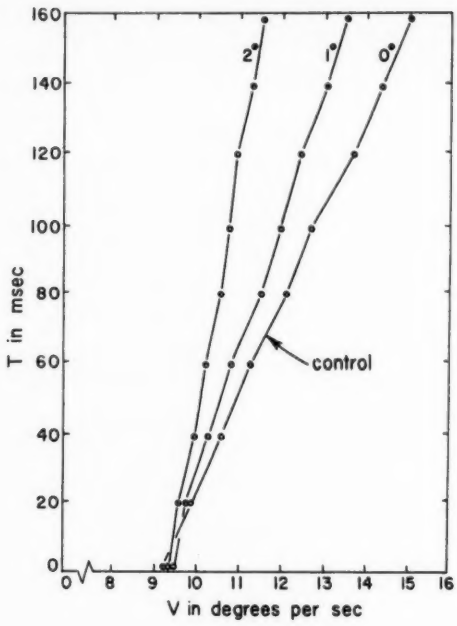

Figure 2. The relationship between the duration of the stationary stimulus (T) and the maximum velocity at which contour could be resolved (V) under each of three conditions of displacement (0, 1, and 2°).

reduced contour facilitation. For the same value of T (160 msec), the maximum velocity at which contour could be resolved was increased from 9.4 to 13.5 and 11.6° for displacements of 1 and 2°, respectively.

Figure 3 shows the effects of displacement upon the phenomenon of contour facilitation for each of three values of T. Facilitation is expressed as the per cent increase in the maximum velocity at which contour could be resolved as a result of the introduction of the stationary stimulus prior to movement (relative to the maximum velocity without the stationary stimulus, $V=9.4°$). Here it may be seen that the longer the duration of the stationary stimulus, the greater the facilitation of contour under each of the three conditions of displacement (0, 1, and 2°). It is also evident that the loss in facilitation as a function of displacement is greater for high values of T than it is for low values.

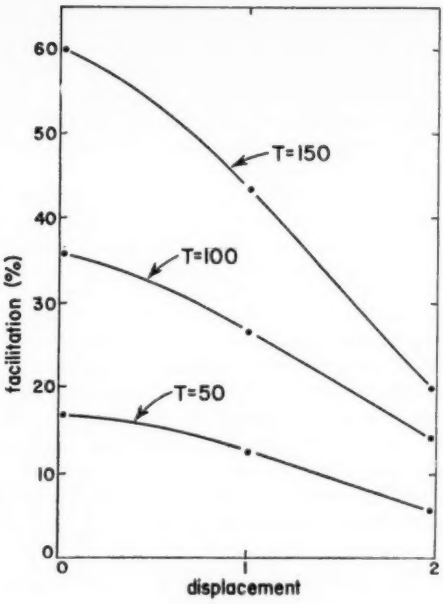

Figure 3. The deleterious effects of displacement upon contour facilitation for each of three durations of the stationary stimulus.

DISCUSSION

The facilitation of perceived contour during stimulus movement has been shown to be related to visual events prior to movement. The edges of a stimulus which can not be seen clearly at a particular velocity can be seen clearly if the stimulus is presented in a stationary position for some critical duration prior to movement. Furthermore, the facilitation is determined not only by the *duration* of the stationary stimulus, but by its *location* in the visual field relative to the place from which movement begins.

An explanation of this phenomenon does not come easily. Nevertheless, there appear to be three important considerations regarding any attempted explanation: namely, the role of the stationary stimulus as a locator of the moving stimulus, the influence of the stationary stimulus upon visual memory of the moving stimulus, and finally, the contribution of the stationary stimulus to the physiological conditions necessary for contour perception. Let us treat each of these considerations.

First, the stationary stimulus may assist *O* in finding the appropriate place in the visual field where the moving stimulus will appear.

It is clear, however, that the first phase of the stimulation sequence does more than locate the moving stimulus. In the control condition the fixation point served to locate the place from which movement began, and yet, facilitation of contour during movement was not noted until the stationary stimulus was introduced. When data obtained in this condition are compared with other data obtained from earlier experiments in which no fixation point was used (Smith and Gulick, 1957), it is clear that the fixation point itself had no facilitating effect upon contour perception. It must be concluded, therefore, that an explanation of the phenomenon of contour facilitation in terms of the stationary stimulus as a locator is unsatisfactory.

Second, it is possible that the stationary stimulus influences *O*'s judgment of the contour of the moving stimulus through visual memory. The stationary stimulus might have allowed *O* a clear view of the stimulus before movement, so that when asked to report upon perceived contour during movement *O* would, in fact, be reporting the remembered contour of the fixed stimulus. This seems unlikely; for if it were so, then displacements of the stationary stimulus should not have affected perceived contour in any systematic way, if at all. Furthermore, the linear relationship between *T* and *V* is not easily fitted within this view.

Finally, let us consider the possible physiological contributions of the stationary stimulus to those conditions suggested as being necessary for contour perception. If we consider that the perception of contour is dependent upon differential stimulation of the visual system, then there must exist some critical difference between levels of excitation in adjacent cortical areas before contour can be perceived. Any conditions which maximize this difference in levels of excitation should facilitate the perception of contour.

At velocities of 15° visual angle/sec the necessary conditions for contour were not met without the presence of the stationary stimulus for some critical duration. Apparently, at this velocity both phases of the stimulation sequence are able to meet the necessary conditions for contour perception during movement even though the latter phase alone is ineffective.

It is suggested that the presence of the stationary stimulus increases the difference in the levels of neural excitation representing the moving contour, and thereby, contour perception is facilitated. Since the stationary stimulus is withdrawn prior to movement, the residual decay of neural activity must be relevant to the facilitation process. The fact that displacements of the stationary stimulus adversely affected contour perception during movement can be taken to support the view that neural activity from the separate phases of the sequence of stimulation are summing in some way. Displacement in the visual field separates the populations of neural elements involved in the first and second phases so as to reduce summation and interfere with the attainment of levels

of excitation which are sufficiently different to allow contour perception.

The deleterious effects of displacement upon contour perception might be explained on the basis of eye movements. If *O* attended to the displaced stationary stimulus rather than to the fixation point, failure to perceive contour during movement might have been due to the fact that *O* did not—or could not—track the moving stimulus with accuracy.

There are, however, two considerations which make this explanation untenable. First, using each of three *O*s, several trials were run after the main experiment in which the fixation point was moved to each of several locations which were different from the place where the moving stimulus first appeared. Although these data were not gathered systematically, they suggest that in situations like those employed in this experiment eye movements are sufficiently fast so as to be of little consequence in disrupting tracking movements. Second, since movement was always from left to right, it appears unlikely that displacements of equal extent to the right and left would produce equivalent effects upon contour perception.

The interpretation of the findings of the present experiment within the framework of the statistical theory of visual activity must be tentative. Nevertheless, such an interpretation seems to accommodate these data more easily than either of the more parsimonious considerations already mentioned.

Further experiments are underway to test some of the statistical concepts so essential to the interpretation offered here.

SUMMARY

Each of eight *O*s observed a small black square (0.5° visual angle) moving horizontally from left to right through a visual angle of 5°. The velocity of movement was increased until *O* could no longer perceive the contour of the stimulus. At this velocity the square was presented briefly in a stationary position prior to movement, and this served to restore contour during movement. The relationship between the duration of the exposure of the stationary stimulus and the maximum velocity at which contour could be resolved was found to be linear. This relationship was determined for each of five conditions. The control condition consisted of having the stationary stimulus appear at the same place in the visual field from which movement began, whereas the other four conditions involved displacement of the stationary stimulus from this place at 1 and 2° right and left, respectively.

Displacement of the stationary stimulus had a deleterious effect upon the perception of contour during movement. The results of this experiment suggest that the perception of the contour of a stimulus in motion is influenced by visual events prior to movement. The facilitation of contour perception during movement is determined by the duration of the stationary stimulus and by its location in the visual field relative to the place from which movement begins.

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The Psychological Record, 9, 153-158.

CORTICAL CONDUCTIVITY: AGE DIFFERENCES AND OTHER FINDINGS[1,2]

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Although the construct *cortical conductivity* has generated a number of fruitful hypotheses that have been supported at substantial levels of significance (Barthol, 1958b; Klein and Krech, 1952; Köhler and Dinnerstein, 1947; Köhler and Wallach, 1944; Livson and Krech, 1955), there is no consensus about the exact meaning of the term. It is the position of this paper that cortical conductivity can best be considered as a state of the central nervous system such that electrical or chemical events in the brain allow communication between different parts of the brain. The communication, which makes possible the interaction of concepts or percepts, is partially dependent on *basal conductivity* (Klein and Krech, 1952) which varies between individuals and, over time, within individuals. Basal conductivity is thought of as a relatively stable state of the brain that varies slowly with persistent general *satiation* (Köhler and Wallach, 1944). Day-to-day activity is assumed to reduce the level of basal conductivity, although in a very gradual way; specific and continuous satiation reduces the conductivity in a local area of the brain, but the effect is not lasting. This paper will describe two experiments designed to test levels of basal conductivity, and also will summarize another experiment designed to test a speculative hypothesis on the relationship between cortical conductivity and reading ability in children.[3]

EXPERIMENT I: AGE DIFFERENCES IN CORTICAL CONDUCTIVITY

Problem

If, following the reasoning suggested by Köhler and the others, satiation reduces basal conductivity as a result of continuous stimulation over a long period of time, then logically, children should have a higher level of conductivity than adults. The phi phenomenon has

[1] Reported in part at the Western Psychological Association meetings at Monterey, California in April, 1958.

[2] The author gratefully acknowledges the work of Miss Blanche Smith who collected much of the data reported in these studies and Miss D. D. Kluppel who assisted in the preparation of this manuscript.

[3] Grateful acknowledgement is made to Dr. Julius H. Stier, Director of Research for the Santa Monica Unified School District, and to Mr. Stewart Ferguson, Principal of the McKinley Elementary School for their cooperation and their willingness to explore virgin fields.

been used as a measure of conductivity (Barthol, 1958b): The theory is that when the time interval between successive presentations of two stimuli is varied, the threshold between movement and simultaneity occurs at shorter time intervals for higher levels of conductivity. If children have higher levels of conductivity than adults, the thresholds for children should occur when the time interval between stimuli is shorter than the time intervals obtained for adult thresholds. The specific hypothesis is that the mean value in milliseconds of the threshold between movement and simultaneity will be smaller for children than for adults.

A second proposed measure of conductivity is the kinesthetic figural aftereffect (KAE). It is suggested that differences obtained on KAE measures are related to differences in cortical conductivity, but the direction of the difference (whether larger or smaller) is not clear. The hypothesis is that there should be a significant difference between mean KAE values for children and for adults.

Method

Measurements on the phi phenomenon and KAE were taken on children (50 boys, 30 girls, ages ten to twelve) and on adults (55 men and 51 women, ages nineteen to forty). The differences between the mean values were tested for significance.

Phi Phenomenon

The apparatus is described by Barthol (1958a). The Ss are in a dark room, seated 12 ft. from two small neon lamps enclosed in boxes. A portion of the glowing filaments can be seen through ¼-in. holes. The lamps are mechanically switched on and off alternately at varying rates of speed. The S reports when he sees movement and when he sees two lights blinking rapidly. A measurement of the time interval between the lights at the point of change gives the movement-simultaneity threshold in milliseconds. Eight measures were taken, using the method of serial exploration.

Kinesthetic Figural Aftereffect

The apparatus and instructions are described in Klein and Krech (1952), and in Barthol (1958b). The measurement used to represent the KAE is the mean difference between estimates of the width of a bar, using touch only, prior to satiation and estimates of the width of the same bar after satiation. The larger the difference, the larger the KAE.

Results

Scores, in milliseconds, for the phi phenomenon threshold are as follows: men, 80.5; women, 83.7; boys, 68.7; girls 69.5. There are no significant sex differences. The difference between adults and children is significant beyond the .001 level.

The KAE scores for children were somewhat higher than for

adults. The difference is significant at the .12 level. This does not warrant rejection of the null hypothesis.

Discussion

The results on the phi phenomenon clearly support the hypothesis that basal conductivity is higher in children than in adults. The implications of the findings go beyond the specific hypothesis: since the prediction made on the direction of the differences had been based on the theoretical discussions of Barthol (1958b), Krech (1949; 1950a; 1950b), and others (Korte, 1915; Wertheimer, 1912), these theoretical positions are given support. Although the physical correlates of cortical conductivity are not clear, the concept appears to be a useful source of testable hypotheses. It is hoped that additional research will specify more exactly the nature of the related variables, and, indeed, the exact meaning of conductivity when used to describe cortical activity.

The meaning of the results on the KAE is not so clear. There is no general agreement about the effect of different levels of basal conductivity on satiation. It can be logically argued that a high level of general satiation (low conductivity) will increase the KAE, since only a small amount of local excitation would be required to completely satiate that area. On the other hand, one could take a point of view stemming from a form of Weber's Law: With decreasing basal conductivity, increasing amounts of local excitation would be required to show any effect. The direction of the difference between adults and children on the KAE could not be predicted. Although the hypothesis that there would be a difference on KAE scores was not supported, it must be noted that this was held to be an indirect test of basal conductivity, while the phi phenomenon scores presumably represented a direct test.

EXPERIMENT II: DIURNAL DIFFERENCES IN CONDUCTIVITY.

Problem

No one has yet offered a theory about the length of time required to decrease the level of basal conductivity through day-to-day living, nor the effects of reduced stimulation on increasing basal conductivity. As a first step in obtaining information about this problem, it was hypothesized that basal conductivity should be higher in the morning, after a night's sleep, than in the afternoon, after exposure to many hours of random stimulation. The specific hypothesis was that mean scores on the KAE obtained in the morning would be significantly different from mean scores obtained in the afternoon.

Method

Each of 40 Ss (20 male and 20 female ranging in age from 19 to 24 years) were tested twice on the KAE, once in the morning as soon after arising as possible, and once in the late afternoon. The Ss were

randomly assigned to the morning and afternoon testing so that half were tested in the morning first and half in the afternoon first. Measurements on the KAE were the same as described in Experiment I above. To fit with the trend of the data obtained in that experiment, the afternoon tests should have shown smaller KAE's.

Results and Discussion

There were no significant differences. The measurements taken in the afternoon were smaller, as predicted, but the null hypothesis could not be rejected. The Pearsonian correlation coefficient between the morning and afternoon KAE measurements was .34, significant at the .05 level. The correlation between the control judgments (estimates before satiation) was .67, significant at the .01 level and surprisingly large when one considers the task. It might be concluded from these results that the KAE is a fairly stable measure (the control judgments), but that there is considerable intra-individual variability (morning and afternoon measurements). Further work must be done to specify more exactly the meaning of KAE measurements.

EXPERIMENT III: THE RELATIONSHIP BETWEEN MEASURES OF READING ABILITY AND MEASURES USED TO INDICATE CORTICAL CONDUCTIVITY.

Problem

The final study resulted from speculation that received strong support from a pilot study. It is well-known that more boys than girls are retarded in reading. An earlier study (Barthol, 1958b) had shown sex differences in measures of cortical conductivity. A chain of reasoning, which need not be detailed here, led to the idea that reading problems might be a function of cortical conductivity. Since the reasoning was tortuous, it seemed appropriate to conduct a pilot study before investing much in such a tentative hypothesis. Phi phenomenon and KAE measures were obtained on 10 boys from the University of California Clinic School who had varying reading achievement test scores. The results were as predicted and were all significant. The phi phenomenon and KAE correlated .64. KAE and reading achievement correlated .86. The phi phenomenon and reading achievement correlated .63. Publication of such results did not seem appropriate: A correlation of .86 is too large, particularly when the reliability of the KAE is considered. A larger study with a more carefully controlled sample was then undertaken. The hypothesis was that measures related to cortical conductivity should be significantly correlated with measures of reading ability.

Method

A sample of 30 boys and 30 girls was selected, matched for age and IQ. Half had normal reading achievement scores and half had scores below normal. Four groups were used, 15 Ss in each: retarded boys,

normal boys, retarded girls, normal girls. Each was tested on the phi phenomenon and the KAE.

Result and Discussion

The data were analyzed in every conceivable way. By chance, some of the correlation coefficients should have been significant. They were not. The results showed that the phi phenomenon and KAE, used as measures of cortical conductivity are not related to reading achievement as measured by standard reading tests. The fact that the phi phenomenon and KAE were not correlated was not disturbing since a maturation effect had been postulated. The data did not show a trend in any direction that would lead to further investigation. Educational psychologists will presumably have to look elsewhere than at cortical conductivity to determine the variables causing sex differences in reading ability in children.

SUMMARY

Three studies, based on theories of cortical conductivity, were conducted. The phi phenomenon and kinesthetic figural aftereffect were used as indicators. The first study tested the hypothesis that the scores obtained by children on the two measures would differ significantly from the scores obtained by adults, since general satiation would have been less for the children. Scores on the KAE were different at the .12 level. Scores on the phi phenomenon, the more important of the two measures, were significantly different at the .001 level, indicating a higher level of basal conductivity for children.

The second study followed the same pattern, only testing for diurnal differences rather than age differences. No significant differences were found.

The third study, designed to test a speculative hypothesis, investigated the possibility of a relationship between proposed measures of cortical conductivity and measures of reading ability in children. The results indicated that there was no such relationship.

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A VISUAL ACUITY TEST FOR INFANTS UNDER SIX MONTHS OF AGE[1]

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The importance of vision for adaptive behavior is due largely to the perception of stimulus configurations—the size, form, and arrangement of objects in the spatial world. Therefore it is not surprising that the origin and development of configurational vision have received much attention in psychological theories, and have been the source of much controversy. What is surprising is that little is actually known about the early development of this function in humans.

An obvious reason for the lack of crucial data is that it is difficult to test infants during the earliest stage of visual development, since they are too helpless to respond actively to objects. Most tests have consequently utilized visual reflexes or physiological reactions to gross visual stimulation (Pratt, 1946). Such tests have indicated the overall sensitivity of the visual system to light, to movement, and to color, but they are not suitable for showing responsiveness to complexities of the stimulation. It seems to be assumed that the deficit is in the subjects rather than the methods, since it is commonly stated that young infants can see only vague light and dark areas, movement, and perhaps color.

Several years ago a path was made through this methodological roadblock by using a stimulus preference method (Fantz, 1956, 1958a, 1958b). The method took advantage of infants' natural tendency to explore visually the environment and to fixate objects or areas of interest. Pairs of objects differing only in form or pattern were exposed for short periods while looking responses were observed. Starting as early as the first month of age, a consistent visual preference was shown for certain patterns. This demonstration of pattern vision at an early age called for further research to determine its extent and its development.

Testing visual acuity is a logical starting place. This performance involves most of the mechanisms underlying pattern and spatial vision, such as central fixation, refraction to give a focussed retinal image, accommodation and convergence for nearby distances, and a fine retinal grain with point-to-point cortical projections. The degree

[1] Presented at the meeting of the Midwestern Psychological Association in Chicago, May, 1959. The research was supported by grant M-2497 from the National Institute of Mental Health, U. S. Public Health Service. It was carried out with the cooperation of the DePaul Infant Home.

of acuity can provide a behavioral measure of the anatomical and physiological maturation of the visual system, while it is little affected by learning and higher mental processes.

The literature reveals few attempts to test acuity in infants under six months. A test reported by Gesell, Ilg and Bullis (1950) consisted of holding a small pellet in front of the infant and noting any reaction to it. A technique used by Schwarting (1954) required the visual pursuit of a fine wire moving back and forth across a lighted area. Finally, Gorman, Cogan, and Gellis (1957) rotated a field of black-and-white stripes around the infant; the occurrence of optokinetic nystagmus indicated the capacity to resolve detail at least as fine as the width of the stripes.

Although Gorman's test is sensitive and reliable, it is not directly relevant for studying form and pattern vision. The nystagmic response involves peripheral as well as central vision, it is reflex in nature, and it probably can be mediated by subcortical visual centers (Halstead, 1947, p. 82). Ablation of the visual cortex in mammals does not impair acuity as measured by optokinetic nystagmus, but it produces complete loss of pattern and spatial vision (Morgan and Stellar, 1950).

METHOD

In order to obtain data comparable to other results involving central fixation of stationary patterns, a visual preference test of acuity was devised and was given a preliminary trial in the present study. Young infants had previously shown a strong tendency to look more at patterns (i.e., checkerboard, bull's eye, and stripes) than at homogeneous objects (Fantz, 1958b). This suggested that the threshold for fine detail might be approximated by using a graded series of striped patterns and finding the smallest preferred over a comparison stimulus.

The test patterns were 6-in. squares of vertical black-and-white stripes, either 1/8, 1/16, or 1/32 of an inch wide. The comparison pattern consisted of stripes too fine to be resolved by the infants. This was used instead of a plain gray field as a simple means of equating brightness.

The experimental chamber was two feet square and 18 in. high. The inside was a uniform blue color, which has a quieting effect on infants (Pratt, 1946). It was illuminated by a 40-watt bulb below the infant's field of view. The infant rested face up in a form-fitting, hammock-type crib below the chamber. The infant's eyes could be observed through a quarter-inch hole in the ceiling of the chamber.

The patterns were one foot above the infant on either side of the observation hole, and were one foot apart between centers. They were attached to a disc which formed the center part of the ceiling. The disc could be turned to reverse the positions of the patterns be-

tween exposures. At the end of a test exposure the patterns were hidden by two window shades which met across the middle of the chamber. A 2-in. hole in the shades served to attract the infant's gaze to the center between exposures, so that when the shades were opened there would be an equal chance of eye movements toward either pattern.

The direction of gaze of the infant was revealed by tiny reflections of the patterns from the cornea. The superposition of one of these reflections over the pupil was the criterion of fixation of the corresponding pattern.

The response time for each pattern was recorded on electric timers which were activated by sensitive push buttons.

The subjects were 21 healthy infants ranging in age from 2 to 21 weeks. They were given six 30-sec. test exposures, two with each of the test patterns in random order. The relative position of test pattern and comparison pattern was reversed for the second exposure of each pair. The scores were summed for the two exposures to cancel position preference.

RESULTS

The subjects were divided into two age groups for the data analysis. Two months was chosen as dividing point because previous research showed changes in pattern preferences at this age (Fantz, 1958b).

The results are given in Table 1. The first column for each group

TABLE 1

PREFERENCE DATA FOR TWO AGE GROUPS TESTED WITH THREE SIZES OF PATTERN, EACH PRESENTED WITH THE SAME COMPARISON STIMULUS

STIMULUS		INFANTS UNDER 2 MO.			INFANTS OVER 2 MO.		
Width of Stripes (in.)	Visual Angle (Minutes of arc)	No. Preferring Each[1]	Mean % of Response Time	p[2]	No. Preferring Each[1]	Mean % of Response Time	p[2]
1/8	36	6	59%	—	8	86%	.01
1/64	4	3	41%		2	24%	
1/16	18	5	50%	—	7	74%	.01
1/64	4	3	50%		1	26%	
1/32	9	4	54%	—	5	56%	—
1/64	4	4	46%		4	44%	

[1] Total N varies slightly, due to inability to complete the testing of some infants on each pair of patterns.
[2] Significance of preference as determined by Wilcoxon matched-pairs signed-ranks test of percentage differences.

shows the number of subjects preferring each pattern of the two paired together. For example, 6 infants under two months gave a higher score for the ⅛-in. stripes, while 3 infants favored the comparison pattern. The relative strength of preference for the two patterns was also calculated for each infant and averaged for the group to give the figures of the next column.

The younger group did not show a significant differential for any size of pattern although there was perhaps some preference for the largest pattern. The infants between two and five months, on the other hand, discriminated both the 1/8-in. and the 1/16-in. stripes from the very fine pattern. The two age groups differed significantly (.05 level) in degree of preference for the 1/16-in. pattern (Mann-Whitney *U* test).

DISCUSSION

These results demonstrate that a visual preference test of acuity is feasible for infants under six months, and that there is an increase in visual resolving power during this period. This increase is to be expected in view of the rapid maturation of the visual system during early postnatal life: The macular area of the retina, essential for acute vision in the adult, is poorly developed at birth in terms of size and density of cones, but has about completed its development by the fourth month (Mann, 1950; Peiper, 1949). Electrical activity of the retina, almost absent at birth, increases gradually during the first year (Zetterstroem, 1951). Myelinization (presumably important for adequate neural functioning in those cases where individual fibers are specialized, as in the visual projections), increases rapidly in the visual centers and pathways from birth to two months (Langworthy, 1933). Histological evidence shows marked changes in the visual cortex during the first months of postnatal life (Conel, 1941). The first appearance, at two to four months, of rhythmic potentials in electoencephalograms from the visual cortex has been interpreted to mean the onset of cortical vision (Smith, 1941).

The second column of Table 1 gives the size of visual angle subtended by each stripe of the pattern at the test distance of one foot. The results suggest that the minimum separable acuity for infants from two to five months of age is about 18 minutes of arc. The best previous performance for infants under six months is 33 minutes of arc, obtained from newborn infants using the nystagmic response to moving stripes (Gorman et al., 1957). For comparison, the standard for adults with normal vision is 1 minute of arc. Although the vision of infants is not acute by adult norms, the basic mechanism for pattern and spatial vision is clearly functional during the first six months.

The specific acuity obtained is not too meaningful taken by itself, since acuity is known to vary greatly with testing conditions (e.g., nature of stimulus pattern, illumination, distance, and state of refraction),

and since the optimal conditions for infants are not known. For example, relative to the optimal distance for resolving fine detail of a constant visual angle, there is much confusion. Most visual tests of young infants have been made at distances of less than one foot, presumably because responsiveness is greater to nearby stimuli. Ling (1942) presents evidence for this, but without equating retinal size of the stimulus at different distances. Gesell et al. (1950) state that the infant only gradually conquers the space in front of him, starting with a distance of several inches at birth. In apparent contradiction, it is established that the neonate is hyperopic rather than myopic, and has poorly developed muscles of accommodation (Mann, 1950; Slataper, 1950). It is believed by some that the accommodative mechanism is nonfunctional at birth (Pratt, 1946). This would place the optimal distance for focussing a clear image, and thus for acuity, at infinity. On the other hand, Peiper (1949) has presented evidence for accommodation in newborn infants, and the overall trend is for decreasing power of accommodation with age.

The visual preference test of acuity provides a research tool for settling issues such as this. A study is in progress that will give comparable acuity measures at different ages, at different testing distances, and with or without accommodation. It is hoped that the knowledge on the maturation of detail vision gained by such studies will give a foundation for understanding broader aspects of visual development, such as the role of experience and learning in perceiving the size, form, and arrangement of objects in the spatial world.

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The Psychological Record, 1959, 9, 165-170.

THE TESTABILITY OF PSYCHIATRIC PATIENTS IN AN OPERANT CONDITIONING SITUATION[1]

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During the past quarter century a considerable body of knowledge has accumulated about the operant behavior of animals. Only recently, however, have there been systematic attempts to analyze operant behavior in humans. In part this has been due to the necessity for developing a satisfactory foundation at the animal level. In addition, it has been due to certain differences between animals and humans as experimental organisms. One major difference has to do with "motivation." With animal subjects, the relation between a reinforcing stimulus (e.g., food) and its supporting operations (e.g., food deprivation) is relatively straight-forward. It is generally agreed that the food-deprived, food-reinforced, lever pressing rat is "working for food." This is not to imply that food-deprivation-reinforcement relations are uncomplicated, but rather that the interpretation of the general nature of the relation appears to be reasonably clear. With human subjects, the relation becomes more ambiguous. The development of suitable experimental reinforcement poses both practical and conceptual problems. Deprivation operations are not readily instituted. Past reinforcement histories complicate the interpretation of those stimuli found to act as reinforcers. The question "Is the subject working for the reinforcement, or for the experimenter, or for both?", a question rarely appropriate at the rat level, almost invariably rises to plague the experimenter who is studying human subjects. Yet in the analysis of operant behavior, the problem of motivation, of incentive, of reinforcement is fundamental. The basic concepts of operant conditioning derive from the concept of reinforcement.

In previous studies of operant motor behavior in psychiatric patients, various kinds of incentives have been used. Lindsley (1956) compared the effectivness of candy, nickles, female and male nude slides, and feeding a kitten. He concluded that ". . . even though some chronic psychotic adults respond at low rates for candy, candy is still the best general reinforcer to use with chronic psychotics that we have discovered to date (p.23)." Subsequently, King, Merrell, Lovinger, and Denny (1957) used candy and cigarette rewards with acute schizophrenic patients, and noted that " . . . low-severity (schizophrenics) have no difficulty in comprehending the task, but they are reluctant to respond in a monotonous fashion for what they consider to be trivial rewards. They tend to reject the task (p. 324)."

1 This research was supported by P.H.S. Small Grant M-1464, matching grants from Lilly and Wyeth Laboratories, funds from the Theano Foundation (Mrs. E. Paul DuPont), and funds of the Pennsylvania Hospital. Dr. Bullock is Research Psychologist in the Psychological Service. Dr. Brunt is Clinical Director of the North Building Service. Per-Olof Therman, M.D., Director of Research, James J. Dixon, Ph. D., Chief of the Psychological Service, and J. Martin Meyers, Jr., M.D. Executive Medical Officer of the North Building Service provided valuable assistance.

The data of Lindsley (1956) and of King et al (1957) indicate that candy is not an effective incentive for all psychotic patients. Furthermore, in Lindsley's studies many of the subjects did not evince a strong interest in money. Two factors may have been operating against money as an effective reinforcer. First, as patients in a state hospital most of the subjects did not have a place in which to spend money, nor did most of them use money in their daily affairs. Second, as a group they were relatively chronic, severely ill patients. For example, in one sub-sample of 22 patients, 10 (45%) were not able to take standard psychological tests. It is possible, therefore, as Lindsley has suggested, that many of the patients were simply too psychotic to be responsive to symbolic rewards, such as money. This latter interpretation seems to make sense clinically, and is supported, indirectly, by the observations of King et al (1957) that "low-severity" schizophrenic patients were disinterested in candy. Bullock & Maline (1958), in studies with retarded and non-retarded subjects, found that the older subjects and those with higher mental ages were more likely to select pennies than candy as their preferred reward. This suggests some relation between "maturity" and the effectiveness of various kinds of stimuli as incentives.

The exploration of the effectiveness of monetary rewards with psychiatric patients was undertaken. The use of money as an incentive poses problems in many psychiatric hospitals. Not all patients are allowed to carry money. Furthermore, the patients must be able to spend their money. To meet these difficulties, knob-pulling responses were reinforced with poker chips worth money. Hospitalized psychiatric patients exchanged their chips for credit vouchers with which they could purchase a variety of items from the hospital Snack Bar (food, candy, cigarettes, magazines, books, stationery, clothing, gifts, cosmetics, etc.). Outpatients, nonpatients, and those inpatients allowed to carry money exchanged their chips for cash.

METHOD

Apparatus

The subjects were tested in a plywood cubicle located in a small building on the hospital grounds. The cubicle contained a sloping wood panel painted white. On the panel were two metal knobs (Lindsley manipulandii) separated by a reward tray. The reward tray was connected to an automatic chip dispenser located behind the panel. The subject sat on a comfortable chair facing the panel. A paper bag for storing chips was on the side wall. Each experimental reinforcement consisted of the delivery of one commercial poker chip.

Procedure

Each patient was first approached by his treating resident psychiatrist. The psychiatrist asked the patients to take the tests, informed the patient that he could win money to spend in the Snack

Bar, and arranged for the first appointment. When the subject arrived at the laboratory, he was taken immediately to the cubicle by the experimenter, who asked him to get comfortably seated and instructed him as follows:

You are going to take a test.

The idea of this test is for you to win as much money as you can.

The way you can win money is by pulling a knob.

During the test you may pull either knob.

When you pull a knob, all you have to do is pull it all the way out and then let it go, like this (the experimenter pulled the right knob and a poker chip fell into the reward tray).

Sometimes chips will fall down. Each chip is worth two cents.

All the chips which come down are yours. You may keep them in this bag if you wish.

At the end of the test you can bring your chips out to me and exchange them for a credit slip which you can use to buy whatever you want in the Snack Bar.

Remember, the idea is for you to win as much money as you can.

You may start now. I will let you know when the test is over (when the subject had made his first response, the experimenter left the cubicle).

The first test of the initial session consisted of continuous reinforcement. Each right knob response produced one chip. The subject was allowed to win 50 chips (with a 15-minute time limit). When the test was over, the experimenter, sitting behind the panel out of sight of the subject, called out, "That's the end of the first test. Please bring your chips out here." The subject brought the bag of chips out into the main room of the laboratory, where the experimenter counted them, told the subject how much he had won, and wrote out the Snack Bar credit voucher. Upon completion of the session, the subject was taken immediately to the Snack Bar by an attendant. The patient's credit voucher was deposited and a purchase made.

Subjects

The subjects were 55 psychotic patients (19 males, 36 females); 19 non-psychotic patients (8 males, 11 females); 35 student nurses (11 males, 24 females); and 9 university students (7 males, 2 females).

RESULTS

The subjects were classified into one of three categories: *not testable* (either would not enter the cubicle or would not pull the knobs), *partly testable* (would pull the knobs, but ceased responding before completing the tests), *testable* (completed the tests).

Table 1 presents the percentages of psychotic and non-psychotic patients in these three testability categories. Since all of the 44 non-patients were testable, their data have not been included.

TABLE 1. THE PERCENTAGE OF PSYCHOTIC AND NON-PSYCHOTIC PATIENTS IN THE THREE TESTABILITY CATEGORIES.

	Not Testable	Partly Testable	Testable
Psychotics (55)	11%	29%	60%
Males (19)	5%	16%	79%
Females (36)	14%	36%	50%
Non-psychotics (19)	0%	5%	95%
Males (8)	0%	0%	100%
Females (11)	0%	9%	91%

Table 1 shows that psychotic patients were less likely to be testable than non-psychotic patients. With one exception, all of the non-psychotic patients were testable. Of the 55 psychotic patients, 60% proved to be testable.

Table 2 presents the relationship between testability and chronological age for the psychotic patients.

TABLE 2. THE RELATIONSHIP BETWEEN TESTABILITY AND CHRONOLOGICAL AGE FOR THE PSYCHOTIC PATIENTS.

	Not Testable	Partly Testable	Testable
Under 20 (4)	0%	0%	100%
20-29 (15)	0%	40%	60%
30-39 (16)	0%	25%	75%
40-49 (8)	25%	38%	38%
50-59 (5)	20%	20%	60%
Over 60 (7)	43%	29%	29%

Table 2 shows that among the psychotic patients, the younger subjects were more likely to be testable than were the older subjects. The relation was significant ($X^2=8$, $p=.01$, $C=.37$). Only 40.5% of the psychotic patients over 40 were testable, whereas 75% of those under 40 were testable.

Table 3 presents the relationship between testability and the duration of illness for the psychotic patients.

TABLE 3. THE RELATIONSHIP BETWEEN TESTABILITY AN DURATION OF ILLNESS FOR THE PSYCHOTIC PATIENTS

	Not Testable	Partly Testable	Testable
0-1 Years (19)	0%	11%	89%
1-10 Years (20)	20%	30%	50%
11 or More Yrs. (13)	15%	54%	31%

Table 3 shows that psychotic patients who had been ill for one year or less were more likely to be testable than those who had been ill for more than a year. The relation was significant ($X^2=9$, $p=.01$, $C=.38$). Though 88% of the psychotic patients who had been ill for one year or less were testable, only 47% of those who had been ill for more than a year were testable.

Though the relation between age and duration of illness among the psychotic patients was not significant ($X^2=1.2$, $p=.20$-$.30$), the data did suggest some interaction. There were 19 psychotic patients who were older than 40. Of these, the 4 who had been ill for a year or less were all testable. Of the 15 who had been ill for more than a year, only 3 were testable.

Table 4 presents the relationship between testability and psychiatric diagnosis for the psychotic and non-psychotic patients.

TABLE 4. THE RELATIONSHIP BETWEEN TESTABILITY AND PSYCHIATRIC DIAGNOSIS FOR THE PSYCHOTIC AND THE NON-PSYCHOTIC PATIENTS

	Not Testable	Partly Testable	Testable
Psychoneurosis (10)	0%	10%	90%
Addiction (8)	0%	0%	100%
Acute Brain Synd. (1)	0%	0%	100%
Schizo: Paranoid (29)	14%	24%	62%
Schizo: ChrUndif(12)	16%	42%	42%
ManicDepManic (5)	0%	60%	40%
Schizo:Catatonic (4)	0%	0%	100%
Schizo:Affective (3)	0%	0%	100%
Schizo: Acute (1)	0%	0%	100%
Schizo: Hebephrenic (1)	0%	100%	0%

Table 4 shows that schizophrenic patients of the catatonic, schizo-affective, and acute undifferentiated types were all testable. Only 62% of the paranoid schizophrenics and 42% of the chronic undifferentiated schizophrenics were testable. All of the not testable patients fell into the paranoid or chronic undifferentiated types.

SUMMARY

Data from 55 psychotic and 19 non-psychotic patients have been presented which suggest that clinical characteristics of psychiatric patients are reflected in their operant performances, specifically, their testability in an operant situation when rewarded with money.

1. Psychotic patients were less likely to be testable than were non-psychotic patients.

2. Psychotic patients with longer durations of illness were less likely to be testable than those with shorter durations.

3. Among the schizophrenic patients, the paranoid and chronic undifferentiated types were less likely to be testable than the catatonic, schizo-affective, and acute undifferentiated types.

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The Psychological Record, 1959, 9, 171-177.

EFFECT OF EARLY ENVIRONMENTAL RESTRICTION ON SUBSEQUENT BEHAVIOR IN THE RAT[1]

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A number of studies testing the effects of early environment on later behavior have all shown that animals raised in unrestricted environments tend to give better performances on a variety of tasks than do animals raised in more restricted surroundings (Dawson & Hoffman, 1958; Forgus, 1954; Hebb, 1954; Luchins & Forgus, 1955). Some of these studies have concerned themselves with a comparison of isolated and unrestricted animals and have gotten the same results. Hymovitch (1952), for example, found that animals reared in isolated cages where only their biological necessities were taken care of, were significantly inferior on problem solving tasks to animals reared in unrestricted environments.

A question arises, however, as to whether mere isolation from sensory and social stimuli, or restriction in physical space is responsible for the differences in performance. The present study is designed to compare the effects of a large unrestricted, stimulus rich environment with a large, but isolated environment and a physically restricted, but not isolated situation. Specifically, we are testing the effects of three different environments: (a) unrestricted, (b) community cages, and (c) isolated, on the behavioral characteristics of emotionality, exploratory behavior, variability, and the ability to formulate and break a set (susceptibility to the einstellung phenomenon).

METHOD

Subjects and Experimental Environments

The subjects for this study were 30 experimentally naive albino rats of the Wistar strain taken from the F.S.U. laboratory colony. They were bred especially for this experiment. The 30 animals were divided into three groups of 10 rats each. Each group contained 5 males and 5 females.

The unrestricted group lived in the relatively free environment described by Dawson & Hoffman (1958). The total area was about 60 sq. ft. which was divided in half, 5 males on one side and 5 females on the other. The environment was 36 in. high and was covered with

1. The author wishes to thank Dr. W. N. Kellogg for his suggestions and invaluable assistance. This report is based on a paper read at the 1957 meeting of the Florida Psychological Association.

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a metal screen. In addition to the large amount of space, the environment also contained alleys, ladders, mazes, and tunnels to add to the animal's experiential background. The community cage group lived in two cages measuring 9½ in. x 7½ in. x 17 in. There were five animals to a cage, males and females being separate. The isolated group lived in individual cages measuring 9 in. x 8 in. x 10 in. Only a food tray and a water bottle were present in each of the cages of the isolated and community cage groups. The physical space available per animal was 31,104 cu. in. for the unrestricted group, 242.25 cu. in, for the community cage group, and 720 cu. in. for the isolated group. When the animals were 21 days old, they were weaned, marked on their ears, and assigned to their respective environments.

Ten days before the start of testing, all animals were put on 23-hour food deprivation. During this time, three females from the isolated group died. Their ailment was diagnosed as pneumonia and an electric heater was added to the environmental room. No further deaths occurred.

Procedure

When the animals were 70 days old, testing began. On the first three days, they were tested for emotionality and exploratory behavior. The test took place on a straight, enclosed runway, 48 in. long, 4 in. wide, and 6 in. high. The top of the runway was covered with a metal screen. Every 12 in. section was separated with a pencil line. Hall's (1934) criterion of the number of defecations in a 4 minute period was

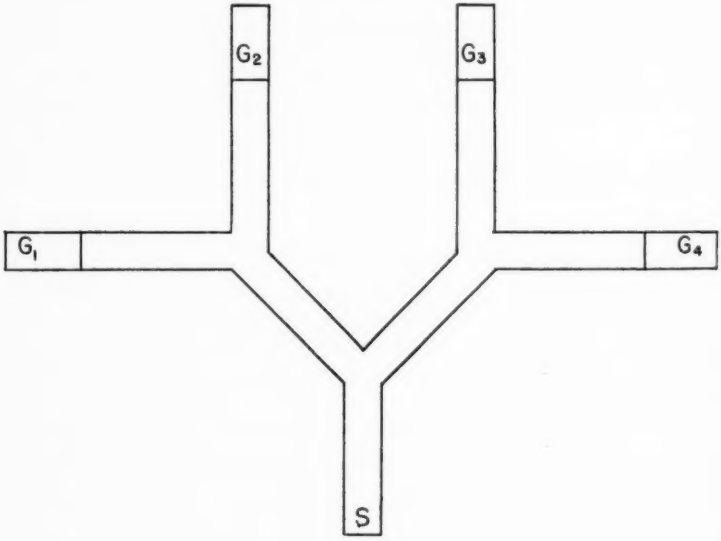

Fig. 1. Variability apparatus.

used as the index of emotionality and Montgomery's (1953a) criterion of the number of lines crossed in a 4 minute period was used as the index of exploratory behavior.

On the fourth and fifth days of testing, the animals were tested for variability on a four unit enclosed Y-maze. The maze was 4 in. wide, 6 in. high, and each arm was 18 in. long. The top of the maze was covered with a metal screen. At the end of each alley was a 12 in. long goal box containing a wet mash of Purina laboratory rat chow. Each of the four goal boxes was equally distant from the starting point. The animals were given five trials per day for two days. The index of variability was the number of times the animal chose an alley different from the one on the preceding trial. Therefore, in ten trials, the maximum variability score could be no more than nine. In addition to the variability scores, time scores for each trial were recorded.

On the sixth day of testing, the forced choice trials of the einstellung test began. The apparatus consisted of a straight alley, 66 in. long, 4 in. wide, and 6 in. high. A U-shaped side alley, 28 in. x 22 in. x 28 in. started out to the right, 22 in. from the starting point. The apparatus was covered with a metal screen. A wet mash of Purina laboratory rat chow was placed at the end of the straight alley. The animals were

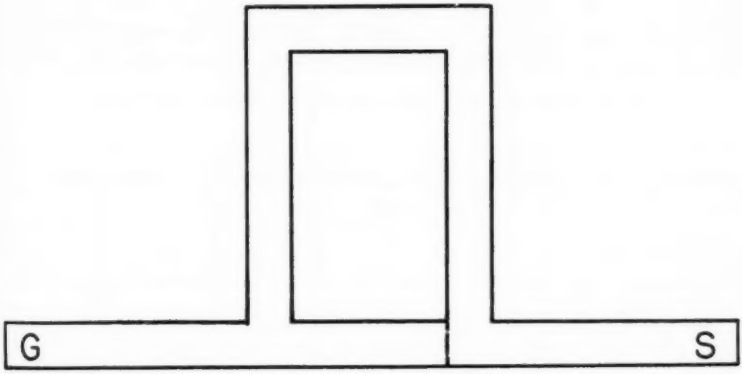

Fig. 2. Einstellung apparatus.

forced to take the longer route to the goal by having the direct path blocked. The index of mechanization (Luchins' (1942) term for fixation or set) was the time per trial. All animals were given 40 forced or mechanization trials at the rate of five trials per day.

The animals were then given 20 free choice trials at the rate of five trials per day. All conditions of testing were the same except that the most direct path to the goal was no longer blocked. A correct re-

sponse was a direct run from start to goal, an error being any deviation from the straight path. The measures of ability to break a set were the number of correct responses and time per trial.

The final three days of testing were a repeat of the emotionality-exploratory behavior test. In order to check Montgomery's (1953b) hypothesis that food deprivation affects exploratory behavior, these final three trials were run while the animals were satiated with food.

RESULTS

Simple analyses of variance were computed for each of the test measures. In the cases of time scores for the mechanization and free choice trials on the einstellung test, the assumption of homogeniety of variance was not tenable. A non-parametric ranking test, the H-test (Kruskal & Wallis, 1952), was therefore computed for these measures. On the emotionality test there was only a single instance of defecation. Therefore, no statistic was computed for emotionality, the implication being that the test did not constitute a genuine emotional situation.

For the first series of exploratory behavior tests, there was no difference between the groups. The results of this test can be found in Table 1. The means refer to the average number of lines crossed in a four minute period per animal.

TABLE 1

MEANS AND STANDARD DEVIATIONS FOR THE THREE GROUPS ON THE FIRST AND SECOND EXPLORATORY BEHAVIOR TESTS

Group	*N*	Expl. Beh. I		Expl. Beh. II	
		Mean	SD	Mean	SD
Unrestricted	10	16.43	8.21	20.33	8.30
Community Cage	10	19.97	12.35	25.10	10.11
Isolated	7	21.62	4.47	17.29	10.33
F		.70		2.08[A]	
p		—		.15	

[A]This *F* is based on adjusted sums of squares in analysis of covariance.

Analysis of covariance yields an *F* of 2.08 which for 2/23 *df* gives a *p* value of .15 for the second series of exploratory behavior tests. In this case, both the unrestricted and community cage groups increased their activity scores by about four to five line crossings per animal while the isolated group decreased their line crossings by about the same amount.

The results of the variability test for both time per trial and variations per animal appear in Table 2. It will be noted from this table that the time per trial for the unrestricted group was faster than that of the other two groups, the results being significant at the 1% level of con-

fidence. For the number of variations per animal, the difference between the groups was significant at the 1% level of confidence, with the unrestricted group making the most variations.

TABLE 2

MEANS AND STANDARD DEVIATIONS FOR THE THREE GROUPS ON THE VARIABILITY TEST FOR VARIATIONS AND TIME PER TRIAL (IN SEC.)

Group	N	Variations		Time	
		Mean	SD	Mean	SD
Unrestricted	10	5.70	2.71	6.59	1.85
Community Cage	10	3.10	1.85	9.39	2.16
Isolated	7	2.29	1.60	10.40	2.80
F		6.09		6.94	
p		.01		.01	

The results of the einstellung test can be found in Table 3. An H-test for mechanization shows the unrestricted and community cage groups to be faster than the isolated group, the results being significant at the 1% level of confidence. For the free choice trials, an H-test for time per trial shows the unrestricted group to be the fastest and the community cage group to be the slowest, the results being significant at the .1% level of confidence. For correct responses, the unrestricted and isolated groups were better than the community cage group, the results being significant at the 1% level of confidence.

TABLE 3

MEANS AND STANDARD DEVIATIONS FOR THE THREE GROUPS FOR TIME PER TRIAL DURING FORCED CHOICE AND FREE CHOICE AND NUMBER OF CORRECT RESPONSES DURING FREE CHOICE ON THE EINSTELLUNG TEST

Group	N	Mechanization Time		Free Choice Corr. Resp.		Free Choice Time	
		Mean	SD	Mean	SD	Mean	SD
Unrestricted	10	7.84	9.15	16.20	5.98	1.89	0.62
Com. Cage	10	8.02	3.70	6.00	6.94	5.20	3.79
Isolated	7	12.99	4.81	13.57	6.83	3.69	1.49
		H=9.20 p=.01		F=6.40 p=.01		H=14.16 p=.001	

DISCUSSION

The lack of defecations on the emotionality test were probably due to the omission of three main features of the Hall Open Field Test. The test was not run on an open field, the total area was not great, and there was no bright light shining down on the animals. In addition, the animals were run while under food deprivation. When run on the

final three days of testing, while satiated with food, they had become so adapted to handling and the testing procedure, they no longer exhibited any emotional behavior.

The effect of food deprivation on the first three days of testing can be noticed on the exploratory behavior scores. In accordance with Montgomery's (1953b) hypothesis, it can be seen that food deprivation had a depressing effect on the number of lines crossed. The deprivation of food obviously had a greater effect on the unrestricted and community cage groups than on the isolated group since these groups had greater opportunity for physical activity such as running, climbing, wrestling with cage mates, etc. When satiated for food, the differences between the groups were much greater, although still not significant.

The results of the variability test are in agreement with the findings of Forgus (1954) and Luchins & Forgus (1955), whose experiment the present study resembles. The unrestricted group, having been exposed to more stimuli in its environment, probably responded to a greater number of stimuli at the choice point than did the more restricted animals. This results in less stereotyped behavior in novel situations.

A similar effect can also be seen in the einstellung test. The unrestricted group learned to make the detour to the goal more rapidly than did the other two groups. When the more direct path was made available to them, they changed their behavior more rapidly than the other two groups in order to take advantage of this route.

From these results, it appears that a large, unrestricted environment has a positive effect on performance for the tests employed in this study. The inclusion of ladders, tunnels, and mazes may also be of considerable importance as these objects provided environmental stimuli which animals living in relatively barren laboratory cages have no opportunity to encounter. As a result, these unrestricted animals exhibited less stereotyped behavior, responding to a greater number of stimuli in novel situations.

A comparison of the community cage and isolated groups shows little difference between them. On some tests the isolated group was better, on others, the reverse was true. On the einstellung test, for example, the community cage group had better time scores on mechanization, but poorer scores during the free choice trials. This would indicate that both physical space and sensory stimuli are important factors in determining the effect of environment on behavior, the effect being about equal for both classes of stimuli.

SUMMARY

Three groups of rats were raised in (a) unrestricted, (b) community cage, and (c) isolated environments respectively. The community

and isolated cages were barren while the unrestricted environment contained ladders, alleys, mazes, and tunnels. All animals were tested, beginning at the age of 70 days, for emotionality, exploratory behavior, variability, and the ability to formulate and break a set. Results show the unrestricted group to score significantly higher on all of these measures except emotionality and exploratory behavior, which did not differentiate between the groups.

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The Psychological Record, 1959, 9, 179-187.

DELAYED REINFORCEMENT, SELECTIVE LEARNING AND HABIT REVERSAL[1]

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Early investigations of delay of reinforcement, reviewed by Pubols (1958), have generally shown that performance during acquisition is a negatively accelerated, decreasing function of the length of delay. More recently, research has been directed towards the possible role of delay in habit formation (Logan, 1952; Seward & Weldon, 1953; Harker, 1956), and the influence of delayed reward upon resistance to extinction. A review of instrumental conditioning experiments on the latter problem (Wike & Kintsch, 1959) led to the generalization that under conditions of relatively massed extinction, partial delay of reinforcement results in greater resistance to extinction than continuous immediate reward.

Three recent studies (Fehrer, 1956; Kintsch & Wike, 1957; Pubols, 1958) have been concerned with the effects of delay in selective learning situations. Fehrer trained three groups of rats to run to one side of a U maze. Group 40 was permitted to drink for 40 sec. in the correct goal box. Groups 10 and 10-D were allowed to drink for 10 sec., but the latter group was confined to the goal box for 30 sec., after consuming the reward (post reinforcement delay). While the groups learned with equal facility, Group 10-D ran significantly faster than Groups 10 and 40 during extinction.[2] Apparently, these mean time scores, reported by Fehrer, were based upon both correct and incorrect runs. It would have been interesting to compare the running speeds on the correct and incorrect trials, separately, for the three groups. Even though Fehrer's groups did not differ significantly on the number of correct turns during extinction, a more detailed analysis of the running times might have disclosed what type of response, e.g., correct or incorrect, or both, was strengthened by delay.

Kintsch and Wike (1957) trained three groups of Ss in a T maze under conditions of no delay, 10-sec. delay on 50 per cent of the correct responses, and 30-sec. delay on 50 per cent of the correct responses. After training, the locus of the food was shifted to the opposite goal box and all Ss received immediate reward. The Ss with 30-sec. delay

[1] Supported by the General Research Fund, University of Kansas.

[2] In a second study, in which the U maze was converted to a runway by blocking off one side arm, Fehrer confirmed this finding as well as demonstrating that pre-reinforcement delay in acquisition had the same effect.

learned more slowly, and had slower habit reversal than the other two groups which did not differ reliably in either acquisition or reversal.

Pubols (1958) studied the effect of magnitude of delay upon the acquisition and reversal of a position response using a Y maze. His groups were delayed for 2 sec., 4 sec., 8 sec., and 16 sec. on *both* the correct and incorrect turns. Following a factorial plan, Ss from each delay group were assigned to one of the four delay conditions during habit reversal. During training, Ss received 80 trials and, in accord with previous findings, the mean days to a learning criterion was a negatively accelerated function of the magnitude of delay. Response perseveration, as measured by the number of errors in reversal prior to making the first correct response, increased with length of delay in training. Mean trials to learn the reversal response to a criterion increased with length of delay in reversal, but was independent of delay in the original training. On the basis of his response perseveration results, Pubols questioned the interpretation of Crum, Brown, and Bitterman (1951) that *variability* in the learning situation produces enhanced resistance to extinction. Rather, he argued, that delay *per se* in his study had led to greater response perseveration. Pubols also questioned the conclusion from the Kintsch-Wike study that delay in training had a significant effect upon reversal behavior, because their design did not isolate this factor from either change of delay or amount of change of delay.

The purpose of the present experiment was to determine the effects of 20-sec. delays on 0, 50, and 100 per cent of the correct responses upon acquisition and habit reversal in a T maze. In the Pubols study, Ss received the same number of training trials in every delay group. Because all trials were free and the non-correction technique was imposed, the various delay groups differed systematically in the number of reinforcements which they obtained in the original learning. To obviate this source of difficulty, Ss had one free and one forced trial daily so that the number of reinforced trials was constant for all groups in the acquisition phase of the present study. Another procedural modification from the Pubols study was that Ss were never subjected to delay following an incorrect turn, while Pubols delayed his Ss after both correct and incorrect responses. Accordingly, his delay groups differed systematically not only in magnitude of delay after a correct response, but also following an incorrect one. Finally, the present experimental design was a three by three factorial plan so that the effects of delay in acquisition upon reversal performance could be ascertained without confounding generalization effects.

METHOD

Subjects

The Ss were 36 experimentally naive male Sprague-Dawley albino rats. In the first replication, the 19 Ss were 60-75 days of age, while in the second replication the 17 Ss were approximately 135 days old.

Four Ss were randomly assigned to each of the nine delay conditions, i.e., all combinations of 0-, 50-, and 100-per cent delay in training and reversal.

Apparatus

The apparatus was a wooden alley-type T maze. The stem was 36-in. by 4.5-in. by 5.5-in. and the first 9-in. section served as a starting box. The side arms were 12-in. long and functioned as the delay boxes. The 10.5-in. by 8-in. by 5-in. goal boxes had their floors raised 0.5-in. above the floor of the rest of the apparatus and were covered by rubber matting. The different sections of the maze were separated by guillotine doors, and the entire maze was covered by hardware cloth. The starting box was painted white, the stem and delay boxes were black, and the goal boxes were dark gray. The maze was placed upon a 12-in. high table and was lighted by two 90 w. fluorescent tubes that were located about 6 ft. above the starting box and were parallel to the side arms.

Procedure

On Day 1, Ss were gentled for 5 min. and all food was removed from the home cages. On Days 2-5, Ss were handled for 5 min. each day and fed mash, made of Purina Egg Chow pellets and water, in their living cages. The 60-min. feeding period used on Day 2 was reduced by 10-min. intervals on subsequent days until the standard feeding period of 30 min. was reached on Day 5. Throughout the study Ss were run after about 22.5-hrs. of feed deprivation. On Day 6 and 7 Ss explored the maze for 10 min. in groups of six. A furniture coaster filled with Purina pellets was present in each goal box. Fifteen min. after exploration, Ss were fed their standard ration in the home cages where water was always available. Additional exploratory runs were given on Days 8 and 9, the only procedural change being that Ss were run in pairs. On Days 10, 11 and 12, Ss had one trial daily in order to determine their preferred direction of turn. Each goal box contained a dish with two pellets which weighed about 0.14-gm. each and S was rewarded, regardless of its direction of turn.

Training took place in the following 18-day period. Each S received a free trial, and at least 10 min. later a forced trial was given in which it was directed to the side opposite from its free trial response. The trials were rotated among six Ss and the non-correction procedure was enforced. Two Ss from each delay condition were caged together and the order of running the three cages followed a random schedule from day to day. The Ss in the 0-per cent group were never delayed; Ss in the 50-per cent group were delayed in a random fashion for 20 sec. on half of their correct turns; Ss in the 100-per cent group were delayed for 20 sec. on all of their correct turns. On every incorrect turn, S was confined to the empty goal box for 20 sec.—a period that was roughly equal to that required to consume the food reward in the correct goal box. All Ss were trained to run to their non-preferred side.

Habit reversal took place in the following 20-day period. The procedure during reversal was identical to that employed in acquisition except that both daily trials were free and the reward dish was now placed in the opposite goal box.

RESULTS

Acquisition

The mean numbers of correct responses for the delay group in the whole acquisition period are shown in Figure 1. These data were analyzed by means of a three by three factorial analysis of variance. De-

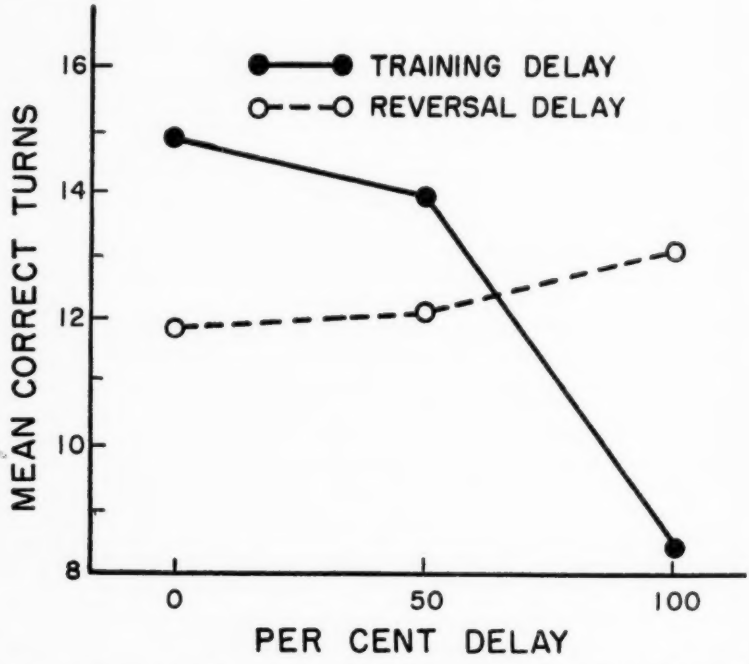

Fig. 1. Mean correct turns in training as a function of percentage of delay in training and reversal.

lay in training was a significant source of variation: $F=8.46$; $df=2,27$; $P<.01$. Delay in reversal and the interaction of delays in the two periods were not significant. Although delay in reversal had not, of course, been introduced as yet, the comparability of these groups is suggested by the presence of only chance differences among the reversal delay groups. The inclusion of delay in reversal in the acquisition analysis duplicated the procedure employed by Pubols (1959). The means for the three training delay conditions were analyzed further by Duncan's test (1955): the 0- and 50-per cent delay groups made

significantly more correct turns ($P<.01$)[3] than the 100-per cent delay group, but did not differ reliably from each other.

Habit Reversal

The mean perseverative responses, as defined by Pubols above, ranged from 1.83 to 3.08 per group. An analysis of variance of these responses disclosed that neither delay in acquisition, nor delay in reversal, nor their interaction were significant sources of variation—$F<1$ in every instance.

The mean numbers of correct turns in the whole relearning period as a function of delay in training and delay in reversal are presented in Figure 2. Figures 3 and 4 display the course of relearning as a function of delay in original learning and delay in reversal, respectively. When the data in Figure 2 were subjected to a factorial analysis of

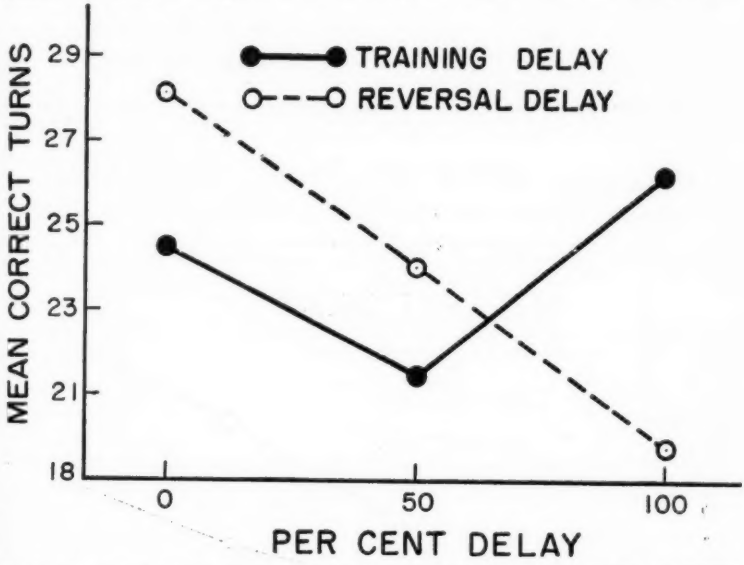

Fig. 2. Mean correct turns in reversal as a function of percentage of delay in training and reversal.

variance, only delay during reversal was found to be significant: $F=7.09$; $df=2,27$; $P<.01$. The application of Duncan's test to means for reversal delay groups revealed that the 0-per cent delay group turned correctly more frequently than the 100-per cent delay group ($P<.01$).[4]

A similar analysis for the data from the last block of trials in re-

[3] In all treatment comparisons and t tests the P-values are two tailed.

[4] When these data were analyzed by means of an analysis of covariance, using the total correct responses in acquisition as the concomitant variable, only delay in reversal was significant: F equals 6.93, df equals 2,26; P more than .01. Subsequent t tests of the adjusted means (Lindquist, 1953, p. 327) also led to the same conclusions as were reached in the original analysis.

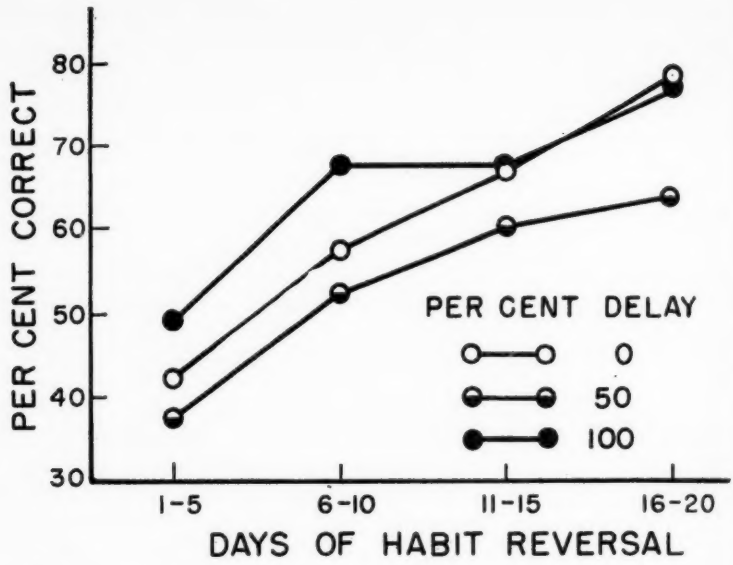

Fig. 3. Percentage of correct turns during reversal as a function of delay in training.

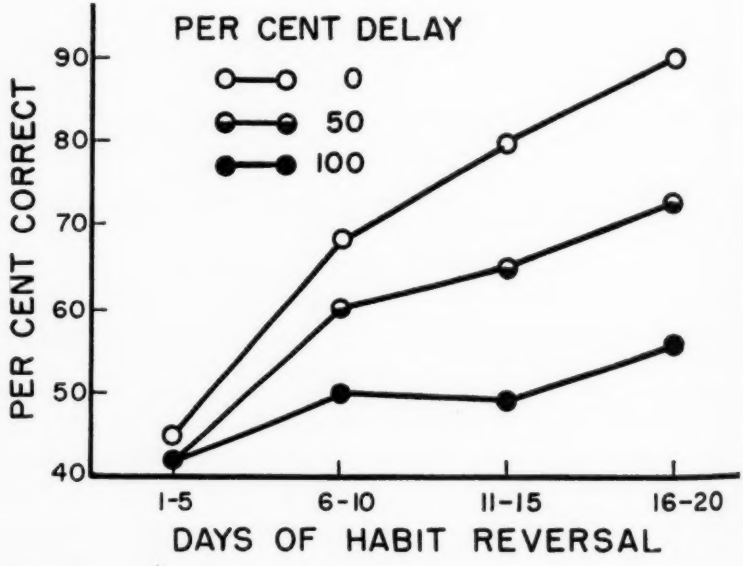

Fig. 4. Percentage of correct turns during reversal as a function of delay in reversal.

learning again led to the conclusion that only delay in reversal was effective: $F=11.77$; $df=2,27$; $P<.001$. At the termination of relearning the differences in means between the 0- and 50-per cent groups and 50- and 100-per cent groups were reliable ($P<.05$), as well as the difference between the 0- and 100-per cent groups ($P<.01$).

DISCUSSION

Summarizing the results, it was found: 1. percentage of delay in training influenced learning; 2. delay exerted no reliable effect on the perseveration responses; and 3. only percentage of delay in reversal affected habit reversal.

The discussion will be limited to a comparison of the present results with those from previous studies and a consideration of the question of whether percentage of delay of reinforcement affects habit strength, H, or reaction potential, E. The present results with percentage of delay show a marked similarity to Pubols' findings on magnitude of delay. In both studies, performance in learning and habit reversal is a decreasing function of delay. The only striking difference between the two studies occurred in the case of the perseveration responses: Pubols found that the number of such responses increased as a function of magnitude of delay in training, while we observed that the frequency of these responses was independent of the percentage of delay. Although the two studies differed in several respects, the most obvious and possibly important difference was that Pubols administered over four times as many training trials.

A comparison of the present results with earlier runway studies of percentage of delays reveals less congruence. The latter experiments, reviewed elsewhere (Wike & Kintsch, 1959), have consistently shown that resistance to extinction is greater after partial delay in training when the training trials are relatively massed. In the present study, neither the number of perseveration responses nor the number of correct turns in reversal was affected by the percentage of delay in training. In Figure 3, it can be seen that the group with 50-per cent delay in training was retarded in reversal, but this trend was not statistically significant. Since none of the runway studies employed a balanced factorial design in extinction, their obtained results could have been due, as Pubols pointed out, to either change of delay or amount of shift in delay. While the reality of runway findings can not be questioned, the attribution of these findings to variability of delay in training must be regarded as suspect.

The present study also bears upon the problem of whether delay is a determiner of H or E. Seward and Weldon (1953) have presented a penetrating review of this issue and the evidence. Although the evidence is not unequivocal, studies involving shifts in magnitude of delay have served to question Hull's view (1943) that delay determines the limit of H. Since performance changes slowly after delay shifts, in

contrast to the rapid changes observed after shifts in amount of reward, Hull's later view (1951) that delay affects *J* as a multipler of *H* also encounters difficulties. Spence (1956) has provisionally proposed that both delay and amount of reward are related to *K* via the classically-conditioned *rg-sg* mechanism. As Seward and Weldon have noted, this view means that the formation of locomotor habits is independent of delay but the conditioning of *rg* is not.

Recently, Spence (1953) has suggested that the complete factorial design may provide a solution to the question: does factor *x* affect *H* or *E*? In the present case, if percentage of delay in training is represented by the rows and delay in reversal by the columns, in an analysis of variance table, then differences among the row means for the relearning responses would indicate a carry-over effect from training, i.e., that delay affects *H*. Since our row differences did not depart from chance variation, no evidence was found to support the assertion that percentage of delay affects *H*. On the other hand, the presence of significant differences among the column means suggests that delay influences some "non-associative" determiner of *E* like *K*. It should be noted that these conclusions, stemming from the factorial analysis rationale are in accord with the conclusions from the shift of delay studies mentioned above.

SUMMARY

Three groups of hungry rats were trained in a T maze position response under conditions of 20-sec. delay of food reinforcement on 0, 50, and 100 per cent of the correct responses. After learning the reward was shifted to the opposite goal box and habit reversal was studied under the three conditions of delay. Performance in the original learning was inversely related to the percentage of delay. The habit reversal performance was an inverse function of percentage of delay in reversal, but was unrelated to delay during training. Perseveration responses were not affected by delay during training. The results were compared to previous findings regarding continuous and partial delay and discussed in terms of behavior theory.

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